Philosophy of Science

VOL. 6 October, 1939 NO. 4

CONTENTS

What Is a Gene? William Marias Malisoff - - 385
What Is Scientific Knowledge? A. G. Ramsperger 390
Causal Imputation and Purposes of Investigation. Lewis A. Dexter - - - - - - - - - - - 404
A Clarification of Critical Realism. Roy Wood Sellars - - - - - - - - - - - - - - - 412
Science and Vagueness. A. Cornelius Benjamin - 422
Value and Fact. Eliseo Vivas - - - - - - - 432
On the Logic of Imperatives. Albert Hofstadter and J. C. C. McKinsey - - - - - - - - 446
The Philosophical Implications of Some Theories of Emotion. M. C. Nahm - - - - - - - 458
Discussion (Correspondence) - - - - - - - - 487
Reviews and Notes - - - - - - - - - - - 490

Copyright 1939, The Williams & Wilkins Company

PHILOSOPHY OF SCIENCE

ORGAN OF THE PHILOSOPHY OF SCIENCE ASSOCIATION

The Editoral Board

WILLIAM MARIAS MALISOFF, *Editor*
New York, N. Y.

ERIC TEMPLE BELL
ALBERT E. BLUMBERG
RUDOLF CARNAP
MORRIS RAPHAEL COHEN
W. W. COOK
HERBERT FEIGL
K. S. LASHLEY
HENRY MARGENAU
H. J. MULLER
L. SUSAN STEBBING
DIRK J. STRUIK
ALEXANDER WEINSTEIN

The Advisory Board

EDGAR ALTENBURG
G. D. BIRKHOFF
NIELS BOHR
MARCEL BOLL
P. W. BRIDGMAN
K. T. COMPTON
E. U. CONDON
W. J. CROZIER
JOHN DEWEY
F. G. DONNAN
L. C. DUNN
ALBERT EINSTEIN
R. A. FISHER
J. B. S. HALDANE
R. G. HARRISON
H. HENCKY
H. S. JENNINGS
K. KOFFKA
W. KÖHLER
K. LANDSTEINER
I. LANGMUIR
RALPH LILLIE
A. O. LOVEJOY
W. MAYER
W. P. MONTAGUE
T. H. MORGAN
CHAS. W. MORRIS
JOSEPH NEEDHAM
J. VON NEUMANN
OTTO NEURATH
F. S. C. NORTHROP
H. A. OVERSTREET
M. POLANYI
G. Y. RAINICH
N. RASHEVSKY
H. REICHENBACH
O. L. REISER
O. K. RICE
A. E. RUARK
GEORGE SARTON
C. E. SEASHORE
H. SHAPLEY
E. A. SINGER, JR.
HENRY B. SMITH
W. F. G. SWANN
R. C. TOLMAN
H. UREY
M. S. VALLARTA
PAUL WEISS
GERALD WENDT
HERMANN WEYL
A. N. WHITEHEAD
N. WIENER
SEWALL WRIGHT
F. ZWICKY

Philosophy of Science

VOL. 6 October, 1939 NO. 4

What Is a Gene?

BY

WILLIAM MARIAS MALISOFF

MY LAST communication dealt with the question of what is an atom. The answer was a proposal to define the atom as the terminus of a special sort of analysis, the atomistic-structural. This form of analysis was presented as a special way of treating a "material" system or situation. A formula was suggested which expressed matter or the material as the product of structure and atomicity, or M = SA. No limitation was placed on the factoring of M into other products, but this particular product was designated as *the structural-atomistic analysis*. The factors, S and A, constitute a *conjugate pair*, so chosen that each member of the pair is meaningless without the other. Atomicity, thus, is not a matter of size or of some sort of ultimateness, but corresponds to some level of being in empirical situations which also reveals structure.

I pointed out that matter itself is a member of a conjugate pair, matter and motion. This fact we can condense in the hyphenated expression, matter-in-motion. In my preferred terminology the inclusion of this consideration converts a *material* system to a *materialistic* one. I said: "the fully materialistic atom with its structure must be dynamic". I now propose to define a gene as the fully materialistic atom of the previous discussion.

Apparently, when motion is introduced, time joins space. A gene, then, can be looked upon as an atom with a history or a life span or a career or simply as an atom with a past, present, and future. All "real" atoms, or atoms in experience, are genes. One might say: there are no atoms, there are only genes. Or, in another way: the atom is an abstraction from the gene. Of course, when we say this, we mean only the narrow, static conception of the atom. It is also possible that on continuing this line of thought, we may find the "fully materialistic atom" not as fully materialistic as we now think it is. The gene would be but one of the factors of something else, let us say a "monad" or a "person" or whatsoever turns up. The gene would be an abstraction from the monad or the person or the whatsoever. And so on. It is conceivable that the *fully* materialistic situation may not terminate with the gene (although for the purposes of the science of today it seems to me to be adequate enough) but it may go on *ad infinitum*. The visioned goal of such constructions would be *the real* and at the same time the *fully* materialistic and *the* ideal as well. Absolute idealism, too, may be "stood on its head". Let that, however, await further study.

A gene, let us say, is the terminus of a *genistic analysis*,[1] and the genistic is the more complete dynamic version of the atomistic. Furthermore, just as the atomistic (more fully expressed) is the atomistic-structural, so is the genistic the genistic-functional.

If my vocabulary is accepted we can state propositions like the following: "all scientific systems are subject to genistic-functional analysis", meaning thereby that when properly analyzed they can show something which will play relative to that analysis the role of an indivisible with a history and something which we can call the *function* of that indivisible with a history, or the gene. Our terminology is based on an analogical ratio, namely, atom is to gene as structure is to function.

We can practice the use of the term genistic (short for genistic-

[1] I might have used the term "genetic" or "generic" but I fear that their special meanings as referring to origins are too strongly fixed. "Genetistic" would have the advantage of being new, but it sounds clumsy. "Genic" still tempts me. "Genistic" was chosen because it is new and parallels the term "atomistic".

functional) by asking questions or making statements as the following: (1) Are there alternative or equivalent genistic analyses? (2) Genistic analyses are always possible. (3) A genistic analysis may lead not only to the so-called genes of biology but also to "life periods of atoms", "half lives of atoms", "semantic histories", "passages", "personal histories" and other relative indivisible with time records.

Since the genistic and the functional are conjugates, we may write analogously to $M = S \times A$ another equation $M' = F \times G$. Read this: the materialistic is the product of the functional and the genistic.

Just as there is no such thing as *the* atom, except as it has arisen by convention to describe something which on a change of analysis came to be looked up as having been *mistaken* for an atom, so there is no such thing as *the* gene. The gene of biology will remain a true gene only so long as it remains part of an analysis which treats it as an indivisible with a history. Just as the discovery of the structure-of-the-atom made it no longer *the* atom, the discovery of functions-in-the gene will make it no longer *the* gene. (Note, please, that "structure-of-the-atom" would have been better expressed as "structure-*in*-the-atom").

To establish this parallel more clearly, I would propose a parallel classification of the atoms as structures and of genes as functions. Thus

proto-atoms	proto-genes
bio-atoms	bio-genes
psycho-atoms	psycho-genes
socio-atoms	socio-genes

will serve to show the effect of atomistic and of genistic analyses in physics, biology, psychology and sociology. It should be apparent also that the present day genes of biology are not the only "bio-genes". An organ, for example, may be considered to be a terminus of a "biogenistic" analysis, provided the analysis rules out the consideration of any functions *in* that organ.

The problem of the individuality of the gene is no simpler than that of the individuality of the atom, nor is it more complex. It would seem to be futile, after having agreed that true atoms

and true genes have no insides, to expect to find so private a thing as individuality by no matter what endless logical manoeuvering. It only becomes a little easier to me to explain why on the "elimination of the observer" you also eliminate the atom (now the gene). The observer is so patently a gene. I think that a really successful elimination of the observer (in the sense of the observing system) would turn out to be the elimination of genistic analysis. Nothing then will have a definite history.

Those who portray the universe-as-a-whole as a gene or a kind of living thing, give us a gene in hopeless search of its function. The term, "history of the universe" can have no meaning for science, since it is as devoid of material sense as the term, "the universe-as-a-whole". Such a Grenzbegriff is lost in an infinite regress. There is no more a last function than a last structure.

Neither is there any reason for a *last* in a series of finer and finer genistic analyses. The biological genes, which in their logical fixity were made the carriers of heredity, are showing alarming indeterminacy. People who are scandalized by what they misinterpret as a naive return to Lamarckianism are simply not aware of the fact that a finer genistic analysis is setting in which will discover functions-in-the-gene. Translated into static terms, the genes will be shown to possess a structure, just as the genes of yesterday, namely the chromosomes, were shown to possess a structure (and internal functions). The controversy concerning heredity and environment is theoretically capable of infinite regression, because genistic analysis is capable of such regression.

The gene is as inaccessible as *the* completed story of the universe. In a relative sense there are innumerable genes and innumerable stories of universes and they are all accessible.

The genes (of heredity) are no more inaccessible theoretically to observations than the story of acorn-to-oak. The latter may also be a gene. As I said about the atom, it merely takes more pointing to point out the genes of heredity than the larger ones of seed-to-plant or seed-to-animal as they function in *their world.* All we require of genes of experience is that they occupy finite intervals.

In the next stage of discussion I may be challenged concerning

the possibility of increasing the number of variables (dimensions) in my description of the materialistic gene. Someone may say: to your space and time add qualities x,y,z, ad libitum, and, then, won't you get something more general than genistic analysis?

I should also like to anticipate the fact that I have not mentioned "substance" as necessary to the notion of materiality. I do not think it is necessary. I think it would be one of those x's, y's, or z's which might at least formally seem to generalize genistic analysis, but will find no exemplification in the world of experience. In other words the x's, y's and z's are either empirically redundant or are non-empirical. They do not do anything in particular that scientific method can seize upon, and are indeed unobservables in the context of our genistic make-up.

The old materialistic atoms which showed *love* and *hate* turned out to have this superfluous or futile quality. Hylozoism adds "life" to its genes merely pro-forma, because such life has no function to perform except to please the author of the theory. The invention of monads served to give individualizing properties to genes but they turned out to be ineffable, incalculable, ad hoc, and fantastically held together by a pre-established harmony, which itself was but a specious assumption.

I repeat, however, that it is as futile to give genes "substance" as to give them "souls". The old time materialist commited a grave *idealist* error in stressing substance because in a sense he was stressing another name for soul. He was adding on individualizing properties or privately held portions of a common property in a situation which rules out individuation at the very start. I do not wish to dismiss the problem of individuation dogmatically, since I intend to discuss it next, but it does not help us to determine what is a gene. The gene is quite as fully materialistic as all of present day science need be.

Polytechnic Institute, Brooklyn, New York.

What Is Scientific Knowledge?

BY

A. G. RAMSPERGER

NO PHILOSOPHER is needed to say where reality may be found. The fool no less than the wise man is in direct touch with real existence at every moment of his waking or dreaming life. To find reality might be a problem for timeless gods beyond the flux of nature—if timeless gods can be said to have problems—but natural creatures encounter reality at every turn. Nor need we look to the philosopher for knowledge. A division of labor having been made between philosopher and scientist, it is to the scientist that we turn for most of our knowledge. Nevertheless, tasks for the philosopher remain and one of these is to make clear what knowledge is, for, to have knowledge is one thing; to know what knowledge is, is another. Science, we may safely assume, constitutes our best claim to knowledge but not all who know their science can say why it is knowledge.

I

First we must guard against a mistaken view about the nature of knowledge. The scientist usually has in the back of his mind a vaguely formulated causal theory of perception according to which a physical world affects the mind through the senses so as to produce sense data—colored shapes, sounds, tactile sensations of solidity. He rarely makes clear to himself whether these directly experienced qualities are located in the physical world, or whether they are appearances inhabiting some doubtful

realm in the mind or in the brain. Do they come to exist only at the end of a process terminating in the brain? Then it would seem to be a mistake to identify the object of experience with the physical world. When we are told that the apparently smooth, brown table is really made up of atoms in rapid motion or of electric charges, or unevenly distributed stresses and strains in a space-time continuum, the conclusion seems to be that the scientist is engaged in describing an unseen world of which our experiences are merely the effects. In that case, the ordinary man who believes his senses, has mistaken a subjective state of his brain, or of his mind, for an objective physical reality and it is the scientist's task to set him right. But if common sense errs, how can the scientist ever correct that error, for on this theory of perception he too is limited to an examination of his own states? If the perceived content depends not only upon what the physical object really is, but also upon unknown physiological and psychological conditions of the subject whose perception it is, no perception need be true to reality, and neither scientist nor layman can get at the truth.

It is evident that a causal theory of perception involves a dualism between what we directly perceive and the physical reality, and that such a dualism would make knowledge of the reality impossible. I assume that science does give us knowledge about a physical world; it follows that the causal theory of perception must be a misconception of the way in which that knowledge is obtained. We must not, then, begin with the assumption that knowledge is gotten by inferring unperceived realities from perceived appearances. Are we then to conclude that the only alternative is the widely held view that science is a systematic classification and description of the objects that we immediately see and touch, and an account of the transformations they undergo?

What is it that we see and handle? There are several answers, each of them true. The answer of one not preoccupied with the problem of perception is that we see and handle shoes and sealing wax and cabbages. Psychologists and philosophers are more likely to reply that they see a brown or a green color, and handle

a bulk that is smooth or rough, warm or cold. There is no ground to deny either of these answers. Whatever else things may be, they are, as William James said, just what they are experienced as. That the same thing can be perceived in two such different ways is no more mysterious than the fact that the illustration in the psychology textbooks can be seen now as a staircase, now as an overhanging cornice.

The apparent inconsistency of assigning to a thing two entirely different descriptions results from the mistaken view that things are of this or that kind apart from their relations to the rest of nature. From this it would follow that if one description is the correct one the other is not about *that* reality but about some other entity, perhaps a subjective image. The fact is that any part of nature is of this or that determinate sort only in relation to this or that selection of circumstances from the natural context in which it stands, and the same thing that is correctly described in one way, when taken in one context, is equally correctly described in another way, when taken in another context.

A human being can be aware of nature only in a context which includes his own perceiving organism, and what a thing is to him will depend upon what *he* is; and what he is, is contingent upon all the circumstances of his heredity and training and upon the interests that motivate his attention at the moment. The greengrocer sees a cabbage; the philosopher, having trained himself to stultify his common sense responsiveness to the object—thinking thereby to guard against reading into the object a subjective interpretation—sees but a round patch of green color. As we shall see later, such deliberate narrowing of one's outlook has its uses, but not because it brings one any closer to what things *really* are. Things really are anything anyone under any circumstances finds them to be. It is easy to overlook the conditions under which experienced objects are what they are. If a housewife says: "Let me have the smaller of those fresh green cabbages", it can be taken for granted that the grocer will understand the spoken words according to established convention, that he will bring to bear upon the situation the usual normal sense organs, that he is familiar with the gestures of pointing that guide

the direction of his glance, and so on. To say that two human beings are similarly constituted and share the social conventions that make communication possible, is to say that they experience things in the same sort of context. When these are the normal and regular conditions of experience one forgets that the nature of things is relative to such conditions. It is only when abnormal circumstances lead to misunderstanding or unexpected experiences that the question is raised whether things *really* are as experienced. The question betrays the false assumption that under the *right* circumstances perception should reveal things as they exist by themselves. Those who assume that the real character of a thing must belong to it apart from any relationships to the rest of nature will find that reality completely eludes them and they are left regretting the fact that truth is forever deep hidden in a well. We may well be suspicious of any assumptions that lead to a doctrine of the unknowable.

But if, given an infinite variety of circumstances and contexts countless characterizations of any part of nature are equally true of it, what comes of the view that knowledge is a description of the objects of experience? Does the man in his cups for whom the cat in the corner becomes a green dragon, have as good a claim to knowledge about the creature as does his sober companion? The answer is that perception, as such, is not knowledge.

Philosophers have usually made knowledge consist in the ability to say which, if any, of the elements in an experience were real and which merely imaginary. To this end they have tried to separate the given from the added interpretation. The given, it was concluded, consists of such logical simples as color, shape, felt roughness. These are the hard data. The problem then is to determine whether these data are real constituents of nature and, if not, to try to determine what nature's constituents really are. That nature has colors and shapes whenever discriminating attention singles them out, is evident. It is likewise evident that nature on occasion brings forth dragons as well as cabbages, unless inebriates have been telling us lies. But the seeing of colors, shapes, dragons, or cabbages is not knowledge. These objects become the subject matter for knowledge only when a question

is raised about their connections and relationships with the rest of nature.

In so far as it is possible to take an experience for just what it is—a consummation, an achievement—it is an occasion for aesthetic contemplation and enjoyment; reality has manifested herself in intuition. Now it is doubtful whether anyone who has lived and learned ever takes an experience for just what it is. The very conditions that make it possible for an experience to be rich in essential content also give it meaning as pointing to other experiences had or to be had. Even the poorest experiences are redolent of their historical associations and prophetic of what is to come. Knowledge is the recognition of the way in which one sort of object is connected with, or enters into the rest of nature. It is a grasp of the circumstances under which nature will exhibit herself in this or that particular form. The drunkard begins to know about cats and green dragons if he can say how many drinks are required to so alter the conditions under which nature exhibited the cat-like characters, that these will metamorphose into the green dragon. His knowledge is deficient when he sees in the dragon a threat to life and limb or believes that his sober companions must also be seeing a dragon. To know an object is to be correct about what it imports. The housewife knows an object to be a cabbage when she is not mistaken in believing that, if it is put to the nose there will be a certain odor, if it is eaten there will be a certain flavor and assuaged hunger, and so on. Thus knowledge is meaning that will bear verification.

II

Scientific knowledge does not differ from everyday knowledge by having to do with a different realm of objects; it differs only in the degree of abstraction it involves, the methods and techniques it employs, and the generality of its results. All knowledge is abstract in the sense that all determinateness of character is the result of the specific circumstances and the perspective of the observer, in relation to whom a certain one of nature's protean possibilities is actualized. But there is abstraction in another

sense. Every object is experienced as a complex unity and by a deliberate narrowing of one's attention gives place to simpler complexes, and finally to seemingly perfectly simple objects such as green or square or hard. While the total aspect of nature in a given perspective is unique and will never recur, more or less similar instances of abstract objects may be repeated. This is possible because not all the conditions, under which an object is what it is, are equally relevant. A repetition of the circumstances sufficient to determine another instance of a green leaf does not require that the entire world stage be set again as it was on the first occasion. The leaf that sprouts from the ivy on a tenement window-sill is not unlike that which sprouts on the ivy on the millionaire's estate. The balloon that is warmed in the sun will expand even though the humidity of the air or the phase of the moon may not be the same as they were on other occasions when this occurred.

In order that changes in certain features of nature may be taken as an index to changes in other features it is not necessary to know all the rest. Otherwise knowledge would be both impossible and useless—impossible because to understand a thing would then require a knowledge of all its connections in the universe, and useless because there would never again be that infinite complex of circumstances to occasion another instance of a certain kind of object. Consequently assertions about nature could have no generality beyond the individual case. The fact that nature shows us recognizably similar objects, and contains things that remain unchanged while others change, is empirical evidence that distinguishable elements of nature are largely conditioned by a limited part of the total context. We find that we can single out and identify objects and determine the conditions of their occurrence with a high degree of accuracy.

Scientific method consists in a technique for the precise determination of the way in which variations in one or more objects determine variations in others. In the exact sciences this takes the form of assigning quantitative (i.e. measurable) values to the variables and expressing their interdependence in equations. This is possible because the qualities of objects, such

as colors and sounds and warmth are functions of the space, time, and mass relationships that hold between the parts of nature. Techniques devised by the scientist have made it possible to so standardize the conditions under which space, time, and mass determinations are made that there is close agreement between different experimenters. This gives rise to the mistaken notion that spatial extension, temporal duration, and mass (and their derivatives such as motion, force, density) are the only properties that truly belong to nature and that these belong to each bit of matter in and by itself. A consideration of the way in which numerical values are assigned to objects will show that designations such as 150 degrees or 50 pounds do not refer to properties at all; they indicate ways in which certain parts of nature are related to one another when we perceive something to be hot or heavy. How does a quality such as hotness come to be spoken of as a temperature of so many degrees or heaviness as a weight of so many pounds? Even the experience of something as large or small is not an experience of a number of feet or inches. A table is experienced as covering an expanse; it is only by the procedure of laying off a foot rule along it and finding that the rule goes into it just five times, that the table can be designated as five feet long. In other words, a numerical expression of length is really a statement of the actual or estimated result of comparing the object, in a specified way, with a measuring unit that was constructed in accordance with certain prescriptions. The determination of a length is the simplest case of associating the "look" of a thing (as small or large) with a numerical expression of the relationship between that object and another.

How do other qualities come to be designated in numerical terms? Immediate experience may present to us the sense qualities expressed by the terms: *cold*, *warm*, *hot*. The difference between these is not quantitative; it does not make sense to say that warm is twice as great a heat as cold. But associated with the change from cold to hot we find other changes. Iron rods and mercury columns lengthen as they become warmer and these changes can be correlated with a numbered scale. Thus 100 degrees Fahrenheit denotes *warm* only through the intermediation

of two steps of correlation: warmth is correlated with size and size is correlated with a numbered scale.

For ordinary purposes there is usually no need to speak of objects in quantitative rather than qualitative terms. A description of the bath water as tepid will do quite as well as being told what the temperature reading is. In either case we are being informed what the consequence will be if the given situation is acted upon in a certain way. To say that the water is tepid is an abbreviated way of saying that a normal person, on putting a hand or a foot into the water, will have the experience described by the adjective tepid. To say that it is 90 degrees is to say what a thermometer will read if put into the water. Those who are informed on the relationship between temperature reading and qualitative feeling can take the former as a sign for what the latter would be if certain acts were performed.

The qualitative and the quantitative statements are equally true but the advantages for science of the quantitative are patent. In place of a set of vaguely distinguished terms such as *cold*, *warm*, *hot*, the number series, with its fractions, affords an infinity of terms and our use of them is limited only by our ability to discriminate marks on a thermometer scale. Distinctions can be made where there is no detectable difference of felt warmth and there is little disagreement among observers on the reading of a numbered scale. A still more important advantage, from the scientific standpoint, lies in the fact that numerical values make possible the formulation of laws which do not merely state that one quality varies with another but provide a means for calculating the precise magnitude of one when the other is given. It is something to know that increased size is associated with increased warmth but one cannot predict from a felt increase in warmth just what the increased size will be, and if one could, it could not be communicated to others with any degree of precision. These advantages are to be gained only if a balloon, say, that is observed to expand as the sun warms it, is dealt with as an instance of the law: the volume of a gas under constant pressure varies directly as the absolute temperature. Here in one brief statement is contained every change in volume associated with

any given change in temperature. If the thermometer shows a five percent increase in the absolute temperature we can infer a five percent increase in the volume. This, then, is the first part of the scientist's task: to abstract from nature those objects whose occurrence or whose changes can be expressed in terms of quantitative laws. This is what is spoken of—somewhat inaccurately—as scientific description. It is not so much a description of things as a statement of the conditions under which they occur or change.

III

The scientist is not content with the discovery of the relationships between measurable variables. Even when this has been formulated in laws there is nothing in these laws themselves that shows why they might not have been otherwise. For example, a *de*creasing temperature might have been associated with *in*creasing pressure or volume. This is to say that laws, gotten by observing what values of a variable are in fact associated with the values of another variable, are not logically necessary. It is on this ground that it is frequently said that science is merely a description of what must remain, for us, irrational brute fact. Such a state of affairs does not satisfy the scientist; he will construct a theory about unobserved entities from whose hypothetical behavior all the observed phenomena would follow. Thus if we suppose that a gas is made up of a large number of freely moving particles—molecules or atoms—then a more rapid motion which, when transmitted to the mercury of the thermometer causes it to expand, will also, by bombarding the walls of the container, increase the volume (the pressure being kept constant). This construction, known as the kinetic theory of gases, is expressed more scientifically as follows: the temperature and volume of a gas are both functions of the average velocity of its constituent molecules. It exemplifies a stage in advance of laws which merely state how one variable changes with another; the kinetic theory explains why the volume increases whenever the temperature increases. When diffusion or other phenomena can also be expressed as functions of the movements of particles, the theory is said to be confirmed.

It is just at this point that important philosophical issues arise. Our thesis has been that scientific statements are not about unobservable physical entities whose action upon the mind causes us to have our subjective perceptions. Rather, they are about the quantitative relationships between the very same objects which are observed as extended, and colored, and warm or cold. What, now, is the status of such things as molecules, atoms, electrons, light waves? They are not observable in the way in which size and warmth are observable; nor do we observe their changes in the way in which a scale measuring a volume is observed to vary with a scale measuring the temperature. An easy but inadequate answer is to say that unobservable scientific entities are constructions invented by the scientist to explain observed functional relationships. This is inadequate because, if by calling them constructions we mean that they exist only in the imagination of the scientist, there would be no reason to suppose that additional predictions based upon them should turn out to be so in fact. Things that do not exist can have no relevance to the occurrence of actual events. But hypotheses about atoms, or light waves, or charges on electrons are important precisely because they have verifiable implications *in addition* to the observed phenomena for whose explanation the hypotheses were first constructed.

The proper question is not simply whether atoms exist, but whether an object, which exists as one kind of thing for a perceiver and which yields certain numerical results when dealt with by delicate measuring instruments, would exist in a certain other way under other circumstances. More specifically, is it probable that an observably continuous object such as a gas or a liquid or an iron bar, would prove to be atomic under an ultra-microscope many times more powerful than any now known? This question remains academic since we cannot, at least at present, actually carry out the suggested procedure. There may even be obstacles that render it forever impossible. It is on this ground that some of the earlier positivists, including Mach, asserted that atoms have no legitimate place in science and predicted that they would disappear from scientific thinking. Mach's insistence upon empirical justification for any assertion about natural fact was sound. If the continued use of atomic, and other such hypotheses

has proved him a poor prophet, this does not mean that we have abandoned his empiricism in favor of metaphysics; nor have we succeeded in setting up conditions under which individual atoms are observed. Mach's predictions did not come true because the analogies upon which the atomic hypothesis rest have many implications other than possible ways of observing single atoms and these implications have been empirically verified.

The atomic hypothesis is based upon analogy with objects which are unbroken wholes when seen from a distance, but which, on nearer approach, become a collection of distinct parts. A stone becomes a granular mass; a heavy fog becomes a collection of widely spaced droplets of moisture; a drop of blood becomes, under a microscope, a motley sea of red and white corpuscles, platelets, and perhaps germs. The behavior and the changes in these objects is usually found to be associated with their corpuscular character. The stone wears away bit by bit; the fog gets thicker or thinner as the droplets grow larger or smaller. Various symptoms of health or illness are the concomitants of the number of blood corpuscles and germs. Now the important point is that, given the notion of composition out of particles, it is possible to apply to a piece of iron or a volume of gas, the laws which are gotten by observing, let us say, marbles in motion, and deduce consequences such as expansion, pressure, effects upon a mercury column, diffusion, etc. The justification for making use of the atomic hypothesis need not wait upon our being able to observe atoms.

Before germs were actually observed with a microscope the germ theory of disease was justified by the success of techniques which it suggested for the treatment of the afflicted. Does this mean that the fruitfulness of the germ theory was evidence of the existence of germs before they were observed? To ask this question is to adopt the false supposition that an object of the sort to be found in the complex of circumstances that includes a microscope and a human observer specially trained to make certain discriminations could exist as that kind of an object independently of, and prior to, the existence of those circumstances. While nature is not the product of the scientist's fancy,

the character of any part of nature is a function of the natural context in which it occurs and the scientist is part of that context. The statement that a patient's blood stream contains germs is an abbreviation. An actual part of nature is characterizable as "a patient with a blood stream" only in relation to an observer with some knowledge of physiology; and to say that the blood stream contains germs is to say that *if* certain things were done with a sample of blood and a microscope there *would be* exhibited objects of the kind the physiologist calls germs. Germs, then, are objective facts in nature in the sense that parts of nature are such that under specified circumstances, those parts are "germ-like". Whatever follows from this fact, according to previously established laws of nature, (e.g. the transmission of disease or its control by sterilization) would still follow, even if the conditions under which nature would exhibit a germ-like character were never fulfilled. The germ theory had its uses before the pioneers of modern biology saw tiny moving creatures under a microscope.

Similar reasoning holds with regard to atoms. When the physicist speaks of atoms he usually thinks of tiny pellets which might conceivably be observed. Using the laws which describe the behavior of larger objects he draws numerous inferences about the pressure and volume of gases, the combining proportions of chemical substances, the diffusion of one substance in another, etc. If these inferences are verified in the laboratory, it no doubt increases the probability that, if certain conditions could be fulfilled, individual atoms would be observed, but there is no reason to regard this as the physicist's aim. What he wishes to do is show that selected features of any given part of nature are functionally related to other observable parts (or to the same part in other perspectives). This may be accomplished through the atomic theory. Thus the theory is a means to an end. Even if atoms were observable their importance would lie in the fact that their behavior explained the connection between phenomena more often found in the ordinary course of life: pressures, volumes, temperatures, lengths of rails. Hence the scientist modifies his concept of the atom in any way that will

enable him to bring more of the observable world into systematic connection. To this end various hypotheses of electrons, protons, and photons are conceived without waiting to establish by direct observation the existence of the atoms of which these various entities are supposed to be the constituents. The essential requirement is that, when laws derived from observable phenomena are applied to these supposed entities, it must be possible to draw inferences which can be experimentally verified. The justification for supposing electrons lies in the fact that the rate of fall of an oil droplet, the tiny flashes of light on a zinc sulphide screen, and many other phenomena are all implied by that supposition.

IV

In conclusion, it may be well to add a word of comment on the bearing of the view here presented upon the issue between realist and positivist. The particular brand of realism which would have us seek the nature of things as they exist independently of human experience, cannot avoid a causal theory of perception. Accordingly, scientific descriptions are regarded as true only in so far as the physical constituents of nature are believed to have been correctly inferred from their effects in the experience of an observer. The positivist, perceiving the difficulty, or even the impossibility, of such a task, asserts that science describes, classifies, and notes the order among the observable data.

There is no good reason to deny the realist's contention that the existence of nature is not dependent upon human experience, but this is a matter of little importance once it is appreciated that the differentiation of nature into specific objects is always relative to other natural objects. A piece of marble is heavy for the muscles of a child; it weighs ten pounds in balance with standard weights; it is yellow in daylight for normal eyes; and the geologist sees it as evidence of what might have been observed by another geologist had he lived aeons ago when numberless creatures were leaving their shells on the ocean bottom. Abandon the search for absolutes—for substances entirely isolated from their contexts—and there is no need for a dichotomy be-

tween things as they appear and things as they are. The positivist, then, is right in saying that the objects directly perceived belong to the world with which science deals.

Both realist and positivist put the emphasis in the wrong place when they conceive science as an attempt to answer the questions: What does the world contain? And which, of all imaginable things, are really there? No special wisdom is needed to find reality. The parts of the world stand to one another in infinite ways and it really contains the myriad of things that every changing perspective brings to light. But there is knowledge only when the pattern of reality as given is a clue to what reality is in the perspective of other times and circumstances.

University of Wisconsin.

Causal Imputation and Purposes of Investigation*

BY

LEWIS A. DEXTER

THERE is a considerable literature about causation.[1] A great many investigators constantly employ the notion of causation in some form. But with the exception of a very few items, these investigators will find little of use in this literature.

In our culture, "scientists" possess a prestige, largely based upon the fact that they are believed to be especially skillful in discovering the "causes" of certain phenomena, and thereby providing means for controlling them.[2] One would therefore expect them to have formulated, more clearly than ordinary people, the principles of causation which guide them.

This does not seem to be the case. Usually, they content themselves with unanalyzed, intuitively-derived, ad hoc conceptions of causation, for each different problem which they encounter. There is no need to convince readers of this journal that, even in matters of scientific method, it is preferable to apply

* I am grateful to Professors J. S. Bixler and T. Parsons, and to Messrs. R. Bierstedt and N. Demareth for criticisms of this paper which have at least helped me towards clearer statement.

[1] See bibliographical note at end of this article.

[2] W. I. Thomas and F. Znaniecki, *The Polish Peasant in Europe and America*, 2nd ed., N. Y., 1927, "Methodological Note," presents a conception of the scientist as one who facilitates possibilities of "social control" a rôle which, professedly at least, our culture often ascribes to him.

the "scientific" type of analysis. There is of course no a priori reason for assuming that ad hoc conceptions are to be deplored; but it is inaccurate to believe, as many men do, that they are acting in terms of a universal causal theory when they are not. Such inaccuracy might be and perhaps is misleading; it is entirely possible that techniques of investigation could be improved, if the underlying conception of cause were more rigorously analyzed.

Philosophers (and scientists, functioning as philosophers) have found two chief stumbling blocks in their analyses of causation. In some form or other, they put forward the argument that "reality" is too "complex" to permit any genuine recognition of causes. In other terminology, this is the argument that causation is so multiple that one might as well give up the idea altogether. (In the social sciences, this viewpoint leads to the argument that social sciences cannot be, because social happenings are so diverse, and to the adoption of a complete scepticism towards any result, obtained by a sociologist, because "you can't be sure.")

The other stumbling-block is also presented in two major ways: (1) Inevitable sequence is not distinguishable from cause and (2) conditions cannot be separated from causes. (Exceptions are sometimes granted, if a perfect experimental situation can be obtained, but in the social and biological sciences, this is excessively rare.) Most of us have heard many times in arguments about war and unemployment, the view "O! capitalism doesn't cause war or unemployment. It just happens to be there," or "O! national 'sovereignty' would never lead to war, except for the presence of capitalism." And the kind of confusion about valid causal imputation presented in such statements is manifested by distinguished political scientists, economists, and sociologists, as well as by politicians and by hoi polloi; and, in relation to different problems, be it added, by allergists and otolaryngologists.

¶ 2. Some of the confusion about causes may be cleared up if we examine for a moment the assumption that an investigator "*discovers*" the "causes" of a phenomenon. This phrase seems to indicate a belief that causes "are" in the universe and part of its

pattern. John Dewey in his latest study of logic,[3] has expressed to a considerable extent what it would otherwise be necessary to say here. He points out that the term "causal laws" is a figure of speech, and that the "category of causation" is logical, not ontological. He makes quite clear the point that any semantical discussion of cause is irrelevant in writing about scientific investigation, because the term does not designate or denote anything at all.[4] Discussions about cause, at least within the world-view characteristic of modern science, are of two kinds: (a) syntactical (logical) about "cause" a term representing a verbal operation or (b) about cause$_p$ (pragmatical, sociological), that is to say the interpretation which individuals place upon the term "cause" within a given context. (A discussion may of course deal with both these levels at once).

Our concern here is obviously not with the metaphysical notions of causation, which introduce such conceptions as essence, ontological character, first cause, etc.[5] nor even with the statements about cause as a universal which linguistically self-conscious logicians of "science" have made, but rather with the ideas about cause, which diagnosticians in any field display when talking about the "causes" of certain more or less specific phenomena.[6]

When faced by a serious problem (i.e. a problem about which he feels something should be done), the ordinary biological organism does do something. Immediately upon determining that one

[3] J. Dewey, Logic, New York, esp. 442–62 "Scientific Laws—Causation and Sequence." The general theme of this work, that philosophy should be a tool for solving problems, is of course adopted in this article.

As far as Dewey's discussion goes, it is unequalled, but it does not cover most of the points raised here, nor attempt to place them formally at least within the framework of semiotic.

[4] From here on the discussion has been greatly guided by C. W. Morris, *Foundations of the Theory of Signs*, Chicago, 1938, (*Ency of Unified Sci.*, 1, 2) from whom the technical employment of semiotic, designate, etc. comes.

[5] Else Wentscher, *Geschicte des Kausalproblems*, Leipzig, 1921, deals chiefly with metaphysical notions of cause.

[6] Linguistically semi-self-conscious individuals, acting upon some assumption about causal imputation, but desirous to prove to themselves that they are not naive, frequently use some circumlocution or synonym for cause, rather than the term itself.

course of action is preferable to another, he makes a causal imputation. (Purely random, trial-and-error actions are so rare, once human organisms have actually formulated a problem, that it is probably accurate to say that all human goal-oriented behavior is associated with causal imputation).

For instance, many people, when afflicted with or fearful of acquiring colds or indigestion, etc., try to avoid the continuance or occurrence of these evils. They therefore alter their ordinary regimens of work, diet, and exercise, thereby showing certain causal imputations about the relationship of work, and/or diet, and/or exercise to colds and indigestion. Similarly, throughout the last twenty years, statesmen and scholars have been making causal imputations about war and unemployment. (In the latter instances, no overt change in what is ordinarily described as "behavior" took place; but the statesman and scholars have verbally advocated certain alterations, thereby indicating their assumptions about causes).[7]

That is to say, when one wishes to prevent an unpleasant, or undesired happening, enhance the pleasure or desirability of a desired one, (or simply arrange for its repetition), one does something to achieve such a result. The basis and justification for believing such activity will be rewarding, furnished by brute sensory experience, has been so adequately discussed elsewhere,[8] that it need not be presented at length here.

We recognize then that people are acting upon causal assumptions, when we see them acting to obtain a desired goal. Cause_p might then be described as precisely that which, if an antecedent, if altered, will produce a result which we wish. (The result may of course be negative, as the stoppage of some phenomenon which we consider bad). This description is simple, but from its utilization may come the means for the solution of often-mooted problems.

First, it means that the scientist, whose concern is with the patterning of phenomena, is entirely correct, when he says that

[7] Q. Wright, American Sociological Review, 3 (1938), 461–74, summarizes the "causes of war" series of the University of Chicago more or less.

[8] Dewey, *op. cit.*

he is concerned with correlation, concomitant variation, etc., but not with causation. He is however entirely mistaken when he states categorically that Science cannot be concerned with causation. (Two interesting studies in the sociology of knowledge are suggested by these statements: (1) how far, in any given cultural context, does even the "purest" and most non-causally-minded scientist organize his problems, researches, and findings, in terms of the socially-significant in his society? (2) how often does the pure scientist, not concerned (according to his own statements) with results, in modern American culture, speak in the name of the great God, Science, about matters of politics and economics, of which, as a pure scientist, he may be profoundly ignorant?) He is not concerned with causation, because he is not planning action; but there is nothing to prevent anyone from investigating, as a basis for action.

Second, and by the same token, those individuals who deny the possibility of National Socialist science, Marxist science, Catholic science, etc. may be mistaken. Obviously, these ideologies are organized around differing sets of values; different problems will surely be investigated by scientists, insofar as they are permitted to function within societies, motivated by such ideologies. Furthermore, even where similar problems are investigated, there will be differing assumptions about what must be retained and what can be altered. Thereby, a different framework of causal imputations may grow up, and a different series of rationalizations link them together. (How far such a possibility does in fact develop, and in relation to which categories of propositions, is again a problem for research by the sociologist of knowledge. If he found that in the specific cases cited above, or in any others, such differences existed, his next problem would be to discover how the differing sciences were intertranslatable).

A clearer example of the above point may be drawn from a controversy current in our own culture, viz. how far is war due to capitalism and/or to political nationalism? The probabilities are that it is due to either, both, both with other factors, neither. In other words, consistent action, predicated upon any of these four assumptions (provided other factors are clearly specified)

might work in getting rid of war. The question to argue is: Which, in terms of other values, would you rather change, if either? If this, and other social and biological problems, were, after a certain point in investigation has been reached, regarded from this standpoint, a vast amount of argument might be avoided.

Third, the definition of cause$_p$ here employed might make it easier to distinguish a priori elements in investigation from those susceptible to modification in the light of further knowledge. If one of one's basic starting points is that capitalism is evil, and must be held responsible for other evils (i.e. war), it is at least time-saving to take this for granted and go on, rather than to spend time and effort undertaking investigation to prove the point. (Or vice versa of course.)

For purposes of emphasizing a point which has been hitherto minimized, this article over-emphasizes to some extent one position, and may seem to adhere to the conventionalist school entirely. There are of course antecedents (such as those which we classify as superstitions) which it is ridiculous to classify as causes. But on the whole common sense, or at least elementary statistics, can detect such errors; trained investigators ordinarily argue about causes, much as the blind men did about the elephant, each being within the correct field of observation. Frequently, every single one of them (within a given framework of values) is right, and the only choice which need be made, is that of choosing from among them one to follow through to the end.

Fourth, scientists should realize that cause, when dealing with contemporary problems, such as war, unemployment, etc., has a signification very different from cause when applied to the development of the solar system or even of exogamy. For, we (plan to do something or) can imagine someone planning to do something about the two former phenomena, far more easily than about the two latter.

This distinction may throw some light on the often-heard assertion that history can not be a social science, because it is descriptive. Our analysis would suggest that historical propositions often are arranged without any goal-orientation in mind, and

therefore proceed upon the assumption of multiple causation. This represents a historical method, differing from that of those scientists who wish to guide action towards the future; but most propositions of anthropology, history, and geology can be placed within a framework where they have relevance to contemplated action.

Lastly, the two stumbling blocks mentioned at the commencement of this article can be removed. Ordinary sequences can be distinguished from causative sequences within a given framework of value; the individual who is in a popular sense a sceptic, i.e. one without any ethical system (if such a one exist), will therefore be in the philosophical sense a follower of Hume, viz. a sceptic. (Many values may be biologically-conditioned, so the present insistence on values probably would not lead to chaos).

In this connection, a further difficulty may be mentioned. In common speech, we are apt to say, "The sun is the cause of warmth," "War is the cause of depression," etc. Actually, of course, substances do not cause substances, etc. What happens is that a certain series of events causes another series of events, i.e. emission of light from the sun leads to sensations in the skin, etc. If this be remembered, Reid's suggestion that Hume must be wrong, because day did not cause night, although one invariably follows the other, is seen for the error it is. If one attempts to describe day and night in terms of actions and reactions between each other, the tangle becomes enormous; but, described in terms of the actions of sun and earth, it becomes relatively clear. This being the case, if one were to attempt to prevent night in a certain area, one would not try to do anything about eliminating day, but would rather try to control earth and/or sun.

Conditions may finally be defined as the negative of causes; factors which are present but are not (from a given standpoint) significant.[9]

Rollins College, Winter Park, Florida.

[9] Sometimes "conditions" is used as meaning basic, underlying, "cause" as immediate, precipitating. It would seem more convenient to call the former basic causes, the latter immediate cause.

BIBLIOGRAPHICAL NOTE

Dewey, *op. cit.*, and Morris, *op. cit.*, are required reading. The above article will appear more significant to anyone who is familiar with the different tenatives towards semiotic made by K. Burke, F. Schiller, H. Reichenbach, H. Lasswell, R. Carnap, I. Richards, A. Korzybski, etc. M. J. Adler and J. Frank, *Crime, Law, and Social Science*, explicitly discuss what kinds of propositions may be taken as causal and what as recording concomitant variations. H. Oliphant and A. Hewitt, Introduction to J. Rueff, *From the Physical to the Social Sciences*, Baltimore, 1929, show the relationship of causal assumptions and formal deduction in the judicial process.

J. Woodger's three articles in the *Quarterly Review of Biology*, 1930–1, "Concept of Organism" deal with the causal postulate (esp. 5; 12–21) and define cause as "the one element essential to the result observed." This definition may be coordinated with the present article; 'essential' and 'result observed' can only be judged in terms of some sort of purpose. See also E. Nagel, *Principles of the Theory of Probability*, Chicago, 1939, 25–6.

J. Somerville in *Philosophy of Science*, 2 (1935) 246–54, "Aims in Social Science" and M. Weber, "Die Objektivität" *Archiv f. Sozialwissenschaft*, 1904 (19) 22–78, supplement the points made above about purpose.

Several studies of the notion of cause and effect in children have appeared of which the latest is from Cornell, by J. Lacey and K. Dallenbach, *Am. J. Psych*, 41 (1939) 103–10.

Chester I. Barnard, *The Functions of the Executive*, Cambridge, 1938, presents a conception similar to the one here given. "The limiting (strategic) factor is the one whose control in the right form at the right place and time will establish a new system which meets the purpose," 202–3.

As for the rest:

In the whole of philosophy, there is scarcely any subject in such utter confusion as as causation. . . . Hume got it into a tangle which has been worse and worse entangled, by subsequent writers, until the latest contributors have essayed to cut the knot by denying altogether that there is such a thing as causation. . . . Few writers treat the subject without contradicting themselves and none without outraging common sense. . . .

This from Charles Mercier's amusing and often pointed *Causation and Belief*, London, 1916, 1, is too true to be funny. His shafts are especially directed against J. S. Mill, *A System of Logic*, (8th ed. esp.) who is the source of most show-window quotations on the matter, and who, he claims, uses "cause" in eighteen irreconcilable ways.

More recent writings include D. Hawkins, *Causality and Implication*, London, 1937, a (not very) neo-Aristotelean refutation of Hume, H. Margenau, *Philosophy of Science*, 1 (1934) 133–48, "Meaning and Scientific Status of Causality," and N. Bohr, *Philosophy of Science*, 4 (1937) 289–96 "Causality and Complementarity" or P. Bridgman, *The Intelligent Individual and Society*, New York, 1938, esp. 33–5, who concludes that "The assumption [cause] . . . is not necessary because physicists have been doing valid thinking without it." (viz. statistical thinking).

A Clarification of Critical Realism

BY

ROY WOOD SELLARS

WHAT I propose to do in the present paper is to clarify critical realism on some crucial points. While such is my primary purpose, I also entertain the hope that logical positivists and pragmatists may be led to see in this form of realism a necessary supplementation to their emphases and insights. I am persuaded that there has been misunderstanding and prejudice at work partly because of verbal phobias in connection with such phrases as the copy-theory, representative perception, transcendence, the subjective, Cartesian dualism, *etc.*, *etc.* So strong have these accumulated prejudices been that, in America at least, there has been a tendency to outlaw epistemology. I do not find the same situation in England for instance. Here it seems at times as though pragmatism and positivism have consciously "ganged up" against epistemology—in my opinion to the hurt of philosophy. I have the impression that the younger philosophers in America are bewildered and are largely drifting. Under these conditions it is the duty of any one who believes that he has a clear-cut epistemology capable of rising above these verbal phobias to seek to present it as precisely as possible.

In a recent article in the *American Sociological Review* I gave chapter and verse for the skeptical attitude of Carnap towards epistemology. In fact, he was frank enough to say that epistemology conveyed no meaning to his mind. Well, I feel here

somewhat as Plato felt towards Antisthenes. But I console myself with the belief that Carnap is a good learner. I do not think that it will be long before he discovers epistemology, or at least the problems whose pressure makes for epistemology. It is evident from his recent work that the questions touching perception are dawning in his consciousness.

In his latest book John Dewey has continued his condemnation of epistemology. On page 525 of his *Logic* I read as follows: "A more detailed examination would confirm the point that has been made more than once, namely, that epistemology, so called, is a mixture of logical conceptions, derived from analysis of competent inquiry, and irrelevant psychological and metaphysical preconceptions. It would confirm the validity of the position that the genuine element in every typical 'epistemological' theory is logical. (Of course, logical must be understood here in Dewey's sense of the term.) It may be added that if one wishes to interpret the 'mental' as that subject-matter of *experience* which has, for the purpose of inquiry and in its conduct, a strictly conditional and hypothetical status (a status that must be given to qualities and meaning while any inquiry is still in progress) there can be no objection. But this interpretation of the 'mental' differs radically from the doctrine that there is involved in knowledge an order of existence which in and of itself is psychical or mental." It is obvious to any old-timer that Dewey falls back on that *impersonal plenum* called experience which is clearly an inheritance from idealism and which has been attacked for so long by Cohen, Rogers and myself, to name but a few. With this plenum goes his rejection of the subjective and the psychical as bound up with Cartesian dualism and with the notion that mental states are intrinsically representative. To me this whole setup is old lumber. As a good evolutionary naturalist I reject Cartesian dualism and do so not by the hocus-pocus of the blessed word experience but by means of the double-knowledge and emergence approach to the mind-consciousness-organism complex.[1] For me the psychical is not naked and shivering but im-

[1] May I refer to my recent article in the *Philosophical Review* called "An Analytic Approach to the Mind-Body Problem"?

mersed in the organism from which it is distinguished by a natural ontological abstraction due to the fact that it is the factor in the brain-state which we do *experience*. It is almost an analytic proposition that each of us can experience his own experience but cannot experience the ontological *context* of that experience. It is quite possible to clear up the situation by means of a good epistemology and a sound categorial analysis. The other point with respect to Mr. Dewey is that no critical realist regards sensations and images as having an intrinsic reference beyond themselves. *Such reference is a property of the complex perceptual experience.* I shall in the sequence try to explain what I mean by this last statement. The gist of the matter is that the perceptual experience includes the rise and operation of the category of thinghood and, with it, denotative reference and characterization.

Now I take this negative attitude of pragmatism and positivism toward epistemology to be a huge mistake; and one very detrimental to the healthy advance of philosophy. Philosophy depends upon persistent analysis, and any attitude which leads to the disregard of basic and unescapable terms and problems is dooming it to sterility. Many of the younger men seem to me to be neither philosophers nor scientists. They are swept hither and thither by the latest winds of fashion. I am persuaded that logical positivism has been wandering in circles because it sought short-cuts and neat formulae and didn't take the trouble to learn what the benighted philosophers were talking about. And it is equally my opinion that pragmatism turned its back too casually upon epistemology, being, apparently, of the belief that this entailed Cartesian dualism and the *naked psychical.* I use in this phrase Mr. Dewey's terminology. The moral I have been indicating is, I suppose, obvious. Pragmatists and logical positivists have had faint hearts. They have hearkened to the skeptics who have told them that no one came out again from the dark wood of epistemology. Well, it is the thesis of the critical realist that he has made a re-analysis of perception which offers a clear path through traditional formulations. Unfortunately, critical realism needs clarification on crucial points. In the famous *Essays* there

was not much more than a program. At any rate I have arrived at an analysis which seems to me essentially correct. I shall propose it as a realistic alternative of which Mr. Dewey seems hardly aware.

I

The guiding principle of critical realism is to keep as near to natural realism as the relevant facts permit. This principle guards it against the purely causal theory of perception as exhibited in Locke and Berkeley. The preliminary is to find out what perceiving seems to be and to take such an inventory as descriptive or phenomenological. Analysis and interpretation must come later. Now is it not a fact that we denote and characterize what we regard as material things external to the embodied self? In looking out of the window I note, and without hesitation, characterize trees and building and persons walking. Perceiving, then, seems to be some kind of direct awareness of things. I note no peculiar act of inference in the perceptual experience though reflection tells me that there has been much interpretation and funding of experience. The recognition of this non-inferential, denotative awareness of things as a feature of the perceptual experience is becoming wide-spread. It is common to the new realists and the critical realists in America, to positivists like Carnap and Feigl, to Dewey, to Stebbing and Ewing in England, etc. Take Dewey, for instance. "The necessary presence of definite objects repeatedly and familiarly employed as means to further knowledge gives the realistic theory its plausibility; a plausibility so great that any other theory seems like a departure from common sense made only to meet the exigencies of some preconceived theory. That stones, stars, trees, cats and dogs, etc., exist independently of the particular processes of a knower at a given time is as groundedly established a fact of knowledge as anything can be."[2] When there is so much agreement where's the difference? The difference arises in interpretation and explanation when some relevant facts are reflected upon.

It is admitted then, quite generally, that we seem to ourselves

[2] Dewey, *Logic*, p. 521.

to be *in some sense aware* of independent, material things in perception. It is quite evidently the job of epistemology to analyze the perceptual experience in such a way as to bring out the beliefs, attitudes and categories operating in sense-perception. The first thesis of critical realism is, then, that perceiving involves more than sensing or, as the modern psychologist might put it, a percept is a complex organization not reducible to sensations as such. There is motor attitude, quasi-belief, funded facts, meanings, expectations. We are far from the atomistic sensationalism of the past.

But, while we seem to ourselves to be in some sense aware of external things and are in our actions clearly concerned with them, notorious facts make the epistemologist introduce distinctions. Must not the sensory appearance be upon reflection assigned to the percipient, not as a naked psychical entity, but in some fashion to be determined by a theory of the status of consciousness in the organism? May not, however, the perceptual claim and denotative reference to external things be valid, and be kept, even though we must give up the natural, yet naive, identification of the sensory appearance with the thing meant and adjusted to? If this distinction is accepted, it leads to an analytic opening up of the perceptual experience with more adequate terminology. We can then speak of the intuition of the sensory appearance and the denotative reference to, and the characterization of, the thing-object. What is at first merged and telescoped together is distinguished by reflection. Is it not because the sensory appearance is intuited that the denoted and characterized object is regarded as in some sense given? Is not the basis of our apparent direct awareness of external things, this guiding function of the intuited sensory appearance? In the same fashion sensory data become *signs* of thing-objects of which we do not regard ourselves as at the time directly aware. It is because of the *function* of the sensory appearance *in* the complex act of perceiving that, in visual perception, we seem to ourselves to be aware of the very surface of the external thing. Let the pragmatist note that I do not assign to the sensory datum any intrinsic representative power. It plays a part in the complex operation of perception

as a guide and sign. And any characterization of the denoted thing-object must take its point of departure from it. That, I take it, is factual. We discriminate, verbalize, conceptualize.

Once embarked on the task of reflective distinction—which, after all, is all that the dreaded enterprise called epistemology is—we are forced to distinguish between intuition, on the one side, and denotative selection or reference on the other. Now this is all that the critical realist means by that dreaded term transcendence. It is a term which expresses the difference between the intuited and the denoted. It signifies that what is intuited is bound up with the percipient, constitutes a factor in his consciousness, while what is denoted is transcendent, external, something which we can be concerned with and mean and act towards, *something on a par with the embodied self*. The transcendent can be denoted by the embodied self, can be pointed to, can be characterized—but it cannot be intuited as the sensory appearance is. At the level of common-sense realism, then, we seem to ourselves to be directly aware of thing-objects on a par with ourselves because we have merged intuition and denotation or—to speak more correctly—we have not yet distinguished them. Private and public, immanent and transcendent are distinctions of an essentially correlative sort. They are developments of reflection but, in my opinion, unavoidable. I mention this for the sake of C. I. Lewis who seems to think that transcendence involves something mystical and incomprehensible which should be mentioned only in a footnote. To repeat, transcendence is a meaning which goes with denotation, with that which is to be characterized, with thinghood, with that which is on a par with the embodied and percipient self, with independence, while immanence is a meaning which goes with that which is intuited or experienced, with consciousness, the subjective, the self. These are distinctions of reflection but well-based and necessary. We denote public, transcendent, material things like Dewey's dogs and cats and stars. At the same time we think them through qualities founded on sensory appearances and on funded facts. Critical realism should, accordingly, be thought of as a reconstruction of naive realism resulting from the reflective distinction

between intuition and denotative characterization. It asserts that the directed awareness of common sense expresses a natural growth in which a sensory appearance is taken to be the thing-object to which the percipient is responding and with which he is concerned. Gradually it is intellectually loosened from this identification and functions as an appearance *of*, a sign *of*, a symbol *of*, the denoted thing-object. It is in accordance with this structure of thought that we arrive at ideas *of*, or concepts *of*, the denoted objects. The point I wish to make again, as against Dewey, is that the critical realist does not assign to sensations and images an inherent representative function. The function is developed in connection with the increasing awareness of the difference between intuition and denotation. I take it that what Morris and others are now calling *semantic meaning* involves what I call denotation.

Again I return to Dewey. "Existences *are* immediately given in experience; that is what experience primarily *is*. They are given *to* experience but there givenness *is* experience. But such immediate qualitative experience is not itself cognitive; it fulfils none of the logical conditions of knowledge and of objects *qua* known. When inquiry occurs, these materials are given *to be* known—a truistic or tautological statement, since inquiry is the subjection of the given experience to operations of inquiry with the intent of institution of objects as known." What a turning in a circle! Objects are *to be known* but never known. In the first place I do not believe that *existences* are immediately given in experience but percepts are in which—as I have pointed out—intuition and denotation, sensory appearance and denotative characterization, are fused. Now I believe that Dewey can make his point here against the new realists but does so only by appeal to the blanket-term experience. In the second place Dewey refuses to take *achieved knowledge seriously as a claim to know denoted existences* and, putting on blinkers, adopts the term warranted assertibility in the process of inquiry. Such is the criticism which Rogers, Lovejoy and I have always made of Dewey. And the reason is obvious, Dewey had no epistemology.

He could see the weakness in both the new realism and in traditional representationalism, and he never undertook the careful study of critical realism.

II

And now, in conclusion, a few additional clarifications.

First of all, the view of perception advanced should not be spoken of as the causal theory of perception. And for this reason: it stresses the total perceptual experience which contains such elements as the denotative selection of thing-objects on a par with the percipient organism, interpretations, expectations, funded facts. We *seem* to ourselves to be aware of material existences on a par with our organic selves. Dewey, Gestaltists, critical realists, all agree here. Nevertheless, analysis justifies the view that the element of sensory appearance is caused in us by the stimulation of our sense-organs. Such is the external condition of its generation in the brain-mind. But such a causal theory of the generation of sensory appearances should not be taken to mean the reduction, *à la Locke*, of perception to the awareness of atomic sensations or ideas. Hence I distinguish between the causal theory of sensations and the traditional causal theory of perceiving. Perceiving is more than sensing.

In the second place, explicit perceptual cognition means the denotative characterization of material things, on a par with the percipient organism, in terms of more or less verbalized predicates founded on sensory particulars. Two points should be noted here. First, I stress sensory particulars as furnishing the material for the characterization of thing-objects. It is obvious that my logical analysis is more like Dewey's than like Santayana's. I am skeptical of essences if more is meant than concepts. Second, the cognitive mechanism of sense-perception has nothing in common with traditional representative perceptionism which meant that sensations are asserted to be like a shadowy double. No wonder that Hume rejected this latter formulation! No, explicit perceptual cognition *uses* sense-material but is itself a denotative characterization of the sole primary object of per-

ception. In this sense, critical realism is a form of directed perception and not of representationalism. It is a directed denotation and an accompanying characterization.

In the third place, it was not until the responsive, interpretative, organizing aspect of perception was recognized that atomic sensationalism was doomed. Those who have despaired of epistemology should take this fact into account. Had Dewey recognized that this development made possible a new deal in epistemology he might well have done the sort of pioneering I undertook. But he and the pragmatists felt the realistic impulse too late in their development. I hold out the olive branch to the younger men. Perhaps critical realism may fuse with empirical realism but *only if* its representatives develop a capacity for epistemological analysis. The manipulation of sentences will get them nowhere.

Finally, critical realism in my opinion has nothing to do with *dualism* of any variety. It rejects Cartesian dualism as I have pointed out for some thirty years. Consciousness is a qualitative factor intrinsic to the organized activity of the mind-brain. In the second place, epistemological dualism was at one time adopted merely as a contrast term to the epistemological monism of the new realists. As I see it, the new realists never distinguished sufficiently between intuition and denotation. They followed Berkeley and Hume instead of re-analyzing perception. When Montague asserts that the critical realist is a Lockian it merely signifies that he has not waked up yet to this novel analysis of perception. Now *from the standpoint of denotation* the critical realist is an epistemological monist. We denote and mean material things and characterize them in terms of predicates founded on intuited sensory material. I hope that this clarification will still the old rumor that the critical realist is a Kantian. Quite the contrary, I do not like the distinction between phenomenal objects and noumenal objects. From the first we are concerned with material objects on a par with the organic self. The act of cognition is very complex but concerned with independent, material things. The world is not constructed or inferred but actively denoted within the matrix of our doing and suffering and

ever more explicitly characterized. It is within the matrix of our doing and suffering that categories arise, categories like space, time and thinghood. Epistemological categories demand ontological categories. *Vive* epistemology and ontology! Let us be philosophers again in the full sense of the term.

University of Michigan.

Science and Vagueness

BY

A. CORNELIUS BENJAMIN

MANY attempts have been made in recent months to throw light on the problem of vagueness. That perfect precision is an ideal not to be attained by any language seems clear. But the obvious fact is that words and sentences in our languages are not so precise as we should like to have them, and we are naturally concerned with finding some sort of device by which vagueness can, in the first place, be detected and measured, and, in the second place, eliminated or reduced to a minimum. I shall not be concerned in what follows with these various special attempts to meet the problem. My task is the somewhat more general one of showing that there are other factors contributing to vagueness, one of which is of particular significance when we leave the realm of ostensively defined symbols and begin to concern ourselves with suppositional symbols, constructs and hypotheses.

The problem of vagueness in connection with ostensively defined symbols seems to involve no particular complications. As suggested by Max Black[1] vagueness in such cases can be measured in terms of consistency of application as exhibited in the responses of a group of individuals. C. G. Hempel[2] points out that vagueness is a function of three variables, a term T, a group of persons G, and a series of objects S. I see no reason to

[1] *Philosophy of Science*, vol. 4, no. 4.
[2] *Ibid*, vol. 6, no. 2, p. 167.

object to these formulations and I believe that they place the problem in a very fruitful context.

But it is well to recognize that further refinements of this formulation are possible, in which other variables are introduced. Two of these may be mentioned. The first may be symbolized by "P", and may be defined as the gesture of pointing by which the attention of the subject is directed to the objects to which the symbol is presumably to be applied. The inadequacies of the ostensive method of defining have been commonly recognized. While pointing is, perhaps, the best method of defining a term it is not without ambiguity. For example, in the experiments mentioned by Black it must have been necessary for the experimenter to point to the series of figures, perhaps even to name them, or by some other means to direct the attention of the subjects to them in distinction from other objects in their sensory fields. Unless the subjects knew the meaning of a gesture of pointing, or the intention of such words as "here", "look at this", "pay attention to these", etc. they would be unable to proceed with the experiment. In the absence of such understanding the subjects might have attended not to that to which the experimenter was pointing but to his finger, or they might not even have attended at all. In other words if the word "pointing" is itself vague in my mind I cannot use an instance of pointing for the clarification of any other term in my vocabulary.

Another factor contributing to vagueness may be symbolized by "C" and may be defined as the complexity of the symbol to be applied. It is not easy to make clear what is meant by "complexity" in this sense. I have attempted to recognize in this factor the generally admitted fact that words like "liberty", "society", "virtue", and "God" are much vaguer than words like "round", "red", "sweet", and "hot". To take a somewhat less extreme case, the word "table" seems more complex than the word "red". The word "red" is, of course, not simple, for it involves reference to spatio-temporal location and to shape, and even as color it has saturation, hue and brightness. But "table" involves all of these and in addition contains reference to material, weight, number of legs, temperature, smoothness, hardness,

and so on. A complex term is defined as a conjunction of simple terms, and vagueness arises when we are in doubt as to whether the complete conjunction of elements is required for the application of the term, or something less than this complete conjunction. In other words, if "p" is defined as "q and r and s and t", and an object is found which is q and r and s but not t, is it to be considered as an instance to which "p" may be applied? Is a table from which one leg is missing still a table? Presumably such a difficulty could be avoided by the adoption of a certain convention, but it is not my task to explore these possibilities. I am interested only in showing that terms for "objects" or "things" are in general vaguer than terms for sense data.

Both of these factors are important. But I wish to call attention to still another factor which is of much more significance in its contribution to vagueness. I am not able to formulate this factor precisely, but it may be clarified along somewhat the following line. It seems that there is an important difference between situations in which I am learning to use a symbol and situations in which I am concerned with the applicability of a symbol already learned. In the former cases I am given the object and the applicability of the symbol to the object, and I am required to determine what the symbol means. In the latter cases I am given the object and the meaning of the symbol, and I am required to determine whether the symbol is applicable to the object. Presumably the problem of vagueness arises only in the latter cases. Now the learning process is characterized, I should suppose, primarily by cases of the former kind. But it seems clear that, in the broader sense, the learning process must include cases of the latter kind as well. In fact it seems hard to believe that the two kinds of cases can really be separated at all. Certainly I can never question the applicability of a symbol unless I have, by some previous situation, determined what the symbol means. But it seems equally true that whenever I determine the meaning of a symbol by reference to its object I have in mind a vague reference to future objects to which the symbol is to be applied. Nevertheless there *is* a conflict of attitudes, and I must decide in any situation which is to prevail.

Let us suppose that a certain term T is vague for me with

reference to a series of objects S. This means, according to Black's analysis, that the term will be admitted to have applicability to certain of the objects, that it will be denied applicability to certain others, and that I shall be unable to decide with reference to its applicability in the case of the remaining objects. At this point I am confronted by a dilemma, which is precisely the expression of the conflict of attitudes mentioned above. With reference to the borderline cases I may do one (but not both) of two things. (1) I may narrow the meaning of the symbol and deny its applicability to these cases. (2) I may broaden the meaning of the symbol and assert its applicability to these cases. There are advantages to each procedure, and the decision is not easy to make. If I adopt the former and make a practice of it I shall find that terms become more and more precise but also that they multiply rapidly. Past cases are then taken as the final court of appeal. A term can never be applied to the future, for its reference is only to cases which have actually entered into the determination of its meaning. A term means only that which causes me to utter it. No error is possible and no term can have advance reference. Every new situation will give rise to a new symbol, and every distinction in subject-matter will be ultimately reflected in a distinction in terminology. But if I adopt the latter procedure and make a practice of it I shall find that terms become more and more vague, though their number decreases. Past cases are no longer taken as the final court of appeal. A term can be applied to the future, for its meaning is defined with reference to the possibility of cases slightly at variance with the past. A term no longer means that which causes me to utter it it but that which might cause me to *choose* to utter it at some future time after a certain deliberation. Error is possible, but only in the sense that I might choose after such deliberation to apply a symbol to an object which exhibits only a very low degree of similarity to past objects.

It will, perhaps, now become clear why this factor contributing to vagueness is so difficult to formulate.[3] The reason seems to be that every symbol (except a pure proper name) is defined with

[3] Carnap seems to have recognized this factor. Cf. his "Testability and Meaning", *Philosophy of Science*, vol. 3, no. 4, section 10.

reference not only to past cases but to future cases as well. By "red" I mean not only the cases of red which I have observed in the past but the cases which I expect to observe in the future. But how like the past cases must the future cases be in order to assure the applicability of the word "red" to them? This is the real issue, and it can be decided only if I have recognized the dilemma mentioned above and chosen one of the two alternatives. If I prefer a vocabulary which is exact but extensive I shall insist that the future cases of red must be *exactly like* the past cases—under which conditions I may find that I shall never experience any cases of red at all. Or if I prefer a vocabulary which is vague but limited I shall insist that the future cases of red may be anything *very loosely resembling* the past cases—under which conditions I may find that everything I experience is red. The point is that *I recognize the claim of the future even when I define my symbol in the first place by reference to observed cases.* I purposely leave a fringe of indefiniteness around every symbol in order to allow me to select at some future time that horn of the dilemma which seems most appropriate. This fringe of indefiniteness contributes significantly to the vagueness of the symbol. It cannot be reduced to zero for then the symbol would have no possible future applications, yet it cannot be made exhaustive of the meaning of the symbol for then the symbol would have no distinguishable content.

The attempt has been made to unite these two incompatible aspects of symbolic reference in the notion of *construct.* A construct is supposed to be both precise and predictive. It is precise because operationally definable and therefore in need of no reference either to vague or to future data. It is predictive because scientifically fruitful and therefore referential to future experimental situations. Now I do not believe that such a combination of aspects is possible. If a construct is strictly a construct, and is operationally definable in terms of past events, it has no future reference at all and no predictions could be made from it. On the other hand, if a construct is not strictly a construct but contains a hypothetical element not reducible to the

past, it may have predictive value. But then it is inevitably obscure. Thus either we demand precision and then resign ourselves to the fact that progress in knowledge consists in fortuitous discovery rather than anticipation, or we forsake precision and then recognize that the future gives hints of itself in the present and that we are therefore able at times to anticipate by means of symbolic devices.

The reason why the notion of *construct* seems to reconcile these two antagonistic attitudes is because it is supposed to refer not merely to the past but to the past which has been *operated upon.* Into the notion of operation has crept the required future reference. But this cannot be justified. For operations are either operations which have been *actually* performed in the past, in which case they have no claim over the future, or they are *possible* operations, in which case they have claim over the future but lack the required precision. This may be illustrated by the attempted operational definitions of *universals*, and of *objects.*

The operational definition of *universals* reduces them to a collection of similar responses on the part of the organism. An individual reacts in the same way to objects a_1, a_2, a_3, *and so on.* But what is the meaning of this "and so on"? Either it describes the way in which the individual has reacted in the past, in which case it is perfectly precise and could be described by a conjunctive proposition consisting of the elementary propositions stating the individual's reaction to a_1 at space-time point s_1, his reaction to a_2 at space-time point s_2 . . . . up to a finite number. Or the "and so on" describes the way in which the individual might (will?) react in the future if confronted with objects of the same kind as a_1, a_2, and a_3. This means either that the reactions of the individual constitute a mere collection of elements, i.e. a collection of behavior acts, in which case the universal is not a "genuine" universal, or these reactions constitute a "genuine" class, in which case the universal cannot be given a satisfactory operational definition and is correspondingly vague as to its meaning.

The same considerations apply to the attempted operational

definition of *objects*. Objects are considered to be constructs of sense data. Now it seems clear that the important issue is whether these sense data are the actual sense data or the possible ones. If they are the actual sense data we do have a true construct, but no predictions can be made; if they are potential sense data we do not have a true construct but a hypothesis, and from this predictions can be made. Russell insists that constructs should replace inferred elements whenever possible. It seems not unlikely that this inferred element which Russell is so anxious to avoid is precisely the factor which must be added to every construct in order to give it predictive value.

Even when the *logical* aspect of operations is pointed out, the notion of constructs is no more adequate. It might be maintained that constructs are made of empirical stuff, but stuff which has been operated upon by logical processes. Consequently even though strictly empirical propositions could have no future reference empirical propositions operated upon by logic could well have. Something like this, I take it, is the point of view of the scientific empiricists. Now such a viewpoint as this does solve the problem, but only temporarily, and only in connection with the proper theory of logical propositions. If future reference is denied to empirical propositions it may well be placed in logical propositions. But then the logical propositions cannot be tautologies. An analytic proposition functions in knowledge in exactly the same way as an empirical proposition which refers only to the past; in both cases the reference is to a decision. In the case of logical propositions the decision is to relate terms in a certain way; in the case of empirical propositions the decision is to apply terms to certain events. But in neither case is there any future reference. Consequently such a theory of constructs can provide for future reference only in case the propositions of logic are genuinely synthetic propositions, i.e. somehow referential to cases which have not yet been examined. Logical constructions cannot be pure inventions but must be in some sense part of nature. If the propositions of logic have no future reference by themselves they cannot have it when applied to empirical pro-

positions which themselves have no such reference; and if they do have such reference they cannot be pure conventions, for they have the capacity to be disconfirmed.

The difficulties to which I have called attention are essentially those involved in the verification theory of meaning, if too narrowly conceived. In its strict form this theory maintains that the meaning of any symbol lies in the consequences which *have been verified*, and such consequences constitute an exhaustive account of the meaning of the symbol, i.e. the symbol is completely reducible to such consequences. This is the attempt to make language faithful only to the past, and is, as was pointed out earlier in this paper, the only theory which succeeds in arriving at a maximum precision. This is the most limited sort of empiricism. A somewhat less strict form would maintain that the meaning of any symbol lies in the consequences which *might or could be verified* (granting the explicit reference to possibility and the consequent element of vagueness which creeps in as a result of this fact), together with some other consequences which I do not think of at the moment but may think of tomorrow (consequently a further element of vagueness), i.e. the symbol is incompletely reducible to its consequences. According to the former theory all scientific explanatory entities are constructs, i.e. symbols completely reducible to their consequences; according to the latter theory all scientific explanatory entities are hypotheses, i.e. symbols incompletely reducible to their consequences. According to the former theory by "molecules" we mean "p and q and r and s", which constitute all of the observation sentences which have up to the present been verified. This conjunctive sentence exhausts the content of the notion "molecule", at least if we wish to be as precise as possible. But according to the latter theory by "molecules" we must mean something more than "p and q and r and s" since we do make genuine predictions from this notion. Therefore this conjunctive sentence cannot exhaust the content of the notion "molecule", and we are committed to an element of vagueness. Whether this additional (irreducible) factor is called an "existential factor" (an "inferred element" in Russell's terms) seems to make

no difference; it is something which cannot be expressed, at least at the moment, in terms of any observation sentences. But to deny its presence is illegitimate. If its presence is admitted by placing it not in the "empirical content" of the notion but in its "psychological content" one may retain his positivism but his positivism is no longer adequately descriptive of scientific procedure. *Vague ideas are part and parcel of science, and any theory of meaning which denies them is not a theory of science.*

If the place of vague ideas in science is denied, the mystery of scientific discovery becomes more than ever a mystery. For discovery occurs only as a result of the firm conviction that no scientific advancement is possible without an unwavering faith in the cognitive value of hunches, vague insights and intuitions. If I understand the theory of constructs correctly it undermines, if it does not actually destroy, this faith. It demands precision above all things. It demands the reducibility of every notion to empirically verifiable sentences, or, even more strictly, to empirically verified sentences. But mere past reference does not suffice; we must and do talk about the future in science. If we blind ourselves to this necessity of future reference and *choose* (by arbitrary decision) not to bother about the future, we can adopt an extreme positivism. But we shall not be fair to science, though we shall be perfectly precise in whatever we say. On the other hand, if we feel obliged to recognize the claim to the future we are committed to an inevitable obscurity in whatever we say. We can attempt to avoid this dilemma by adopting a straddling position—constructionalism, conventionalism, operationalism—but each of these positions when subjected to examination reveals exactly the same duplicity; to the extent to which it recognizes only the past it has no future reference, and to the extent to which it has future reference it is not reducible to the past. These straddling positions clarify the problem only by attempting to locate the future reference in something intermediary between the past and future as ordinarily understood, i.e. in operations. Now operations are capable of description, and to this extent we have achieved a certain clarity. Furthermore, they seem to have future reference (at least *possible*

operations do), and thus we seem to have met that part of the problem. But when operations are carefully examined they prove to exhibit the same duality—past and future operations, particular and universal operations, actual and possible operations, habitual responses as a finite series of past reactions and habitual responses as tendencies to respond. The problem is solved only because we stop the analysis at a certain point.

University of Chicago.

Value and Fact

BY

ELISEO VIVAS

I

THERE can be no possible quarrel with the assertion that the phenomena of value are amenable to scientific treatment if the statement is taken in an obvious sense, for the act of valuation is a fact of human experience, empirically observable in the same way as any other fact. There is therefore no reason why we should not deal with certain aspects of the phenomena of value as we deal with other empirical phenomena. To deny this would be to deny scientific status to sociology, anthropology, and psychology. Sometimes one encounters such denials, but they need not be taken seriously, for they are obviously grounded on a very arbitrary and narrow definition of science.

It is a fact that a large number of people value coffee and some value prestige, and some the opera and a large number of them value swing music and baseball. These are facts that can be ascertained quite objectively. It can also be ascertained that men are willing to give up some of their values in order to retain others. It is in other words a fact that there is a certain degree of systematic interrelationship among the values held by the members of a social group. To seek to find out what men actually value, how their values are interrelated, and how they change, doing so with a modicum of exactitude and clarity, is possible, and in a rapidly changing and complex society is essential for the regulation of practical conduct.

While not many philosophers will object to these remarks, perhaps the majority will point out that they are rather obvious, and misguided if they are meant to deny that there are a number of problems underlying the purely descriptive account of value with which scientific method is not able to deal. These others are the problems which as philosophers we should be interested in, and they center around the nature of value and the reason why we should value some things and not others. Solutions to these problems can be found, but they demand a method different from that employed in the empirical sciences.

The argument of this paper is directed against that thesis. If scientific procedure is understood to include the task of evolving definitions and constructing hypotheses which however remain anchored to verifiable facts—and no philosopher denies that these tasks are the concern of the exact sciences in their own domain—there is no exclusively philosophic problem of value, although on a Baconian conception of science there undoubtedly is. The full and adequate proof of this thesis would lead us out of the realm of value theory and into the thickest of the quarrels between metaphysicians and positivists. Within the field of value theory itself, however, the attempt can be made to show that an empirical definition of value is fully capable of performing all the tasks that definitions may legitimately be called on to perform; and that judgments of value do not differ from judgments of fact, but are indeed judgments of fact in the sense that they are verifiable by means of the same kind of operations which serve to verify any other factual judgment. From this it follows that they have the same compelling character that any other judgment of fact has. This is to say that judgments of value are objective, and when the conditions governing scientific judgments are abided by, value judgments claim the same type of assent as judgments in physics or in biology. Since the weakness of an empirical value theory has always been its failure to account for the compulsiveness of the value judgment, its inability to deal with the "ought" altogether, if this defect can be remedied a scientific theory of value is exhaustive and leaves no room for metaphysics. A metaphysical theory dealing in its

own terms with the same material may still be possible; but it cannot claim to be exclusively valid.

2

A scientific theory of value must of course start with an empirical analysis of the act of valuation, which is generally admitted to entail the psychological approach. But even within the compass of a psychological approach there is still a great deal of conflict as to exactly where is the locus of value. Some thinkers place it in the desire, or appetition, or interest itself, which makes the object valuable; and others in the relation between a desiring self and an object; and still others in the desired object. To review the merits of the competing doctrines would take us too far afield. For the purposes of this discussion it is advisable to accept a definition of value without attempting to justify it otherwise than by demonstrating that it can satisfy the demands made of it. Value, let us therefore assume, is a tertiary character of objects, when objects enter into a relational complex with a desiring self. But this definition is not free of ambiguity, for it can be taken with realistic bias, as my teacher, E. B. McGilvary, takes it, or it can be taken with an instrumentalistic bias. According to Mr. McGilvary we discover the characters of objects from a standpoint or in a perspective. But we are not factors contributing to their presence in the objects. This is the way of "perspective realism." And it entails, I am afraid, at least one difficulty which I can not resolve satisfactorily. For the realist finds the value in the object in a perspective. The word "find" is used literally, and implies the denial that the mind contributes anything to what it finds. It *selects* the value which the object possesses from the standpoint which the mind happens to occupy. From other standpoints—spacial, temporal, or psychological—other aspects of the same thing will be selected. But if the characters of objects reside in them, and all the mind does is select them from a perspective, how can we also say with the perspective realist that characters are not in the objects-by-themselves? "Selection" or "discovery" assumes the pre-existence of that which is selected or discovered. For this reason

it seems better to hold with Mead that the value of the object arises out of the interaction of the desiring self and the desired object. The value is not in the object for the self when the self does not desire it. The organism of course has the interests it has because it has developed in the sort of environment in which it did. But the character the environment has for one organism it may not have for another. It must be emphasized however that value is objective for the valuing organism; and on this fact, but not solely on it, is grounded the claim for the objectivity of the judgment.

It is very easy to confuse this view with the view that value resides in the relation between a valued object and a valuing organism. Objective relativism holds emphatically to the view that value is an objective character. The difference between the two views is sharpened when attention is called to the distinction, neglected by the relational view, between a relational complex, its terms, and the relation between the terms. Value resides in the outside or object-term of a relational complex which is a valuing situation, the inside term of which is the self. That in the cigar which fulfills a need or appetency of mine is its value for me.

It is also easy to create difficulties for one's self by insisting that we do not really value the bouquet of the cigar but the satisfaction that the cigar gives us. This is what L. A. Reid does, in his chapter on value, in his book, "A Study in Aesthetics." This confusion is possible because one can value the satisfaction as well as the bouquet. Those who value the satisfaction which they get from objects and not the objects themselves are hedonists. They value objects as instruments of satisfaction and value the satisfaction for its own sake. But it is possible at least for some men to value objects independently of the satisfactions they produce. In such cases we find objects valuable because there is a need in ourselves for them, but neither the need nor the satisfaction which its fulfillment produces need be an object of awareness. It is to be doubted whether Fluffy's love of milk is something of which that proud feline is aware, though she is well aware of the milk she loves. It is the milk that has value

for her. And similarly poetry possesses value for a man who craves it, as whiskey does for another, and power for a third. They may value poetry or whiskey or power objectively, and not in anticipation of the satisfaction the possession of the objects brings with it. But there are those who value poetry as some drinkers value their liquor, for the sake of the jag it gives them. More often than not it happens that when an object of interest is once found to be capable of producing satisfaction, both the original object and the satisfaction itself are merged into a complex valued object, since objects shift as attention or discrimination does. Whether one values the object or the satisfaction its possession produces or both, seems to be a question of types of people. Some persons seem to be more objective than others, some more subjective. But the largest number, I venture to guess, probably value the satisfaction and the object in a confused way without ever considering seriously what is it that they value. On being questioned they usually speak, I will own, as hedonists. But the reason for this is undoubtedly that hedonistic psychology is deeply ingrained in our language habits. In any case theory must distinguish between valuer and object valued even though the object be within our own skins.

3

Objective relativism has also been reduced to pure subjectivism, as Plato did in the Theaetetus. But the reduction is possible only on the basis of assumptions which are in direct contradiction with ascertainable facts. An elucidation of this confusion enables us to grasp with ease the sense in which the thesis of this paper is to be interpreted.

The first step towards the elucidation is to reiterate that the objective relativist is not looking for objective values residing in "objects-by-themselves." When objectivity is thus interpreted an exhaustive disjunction is made between it and subjectivity, and the denial of one involves the assertion of the other. But the value with which the objective relativist is concerned is a character of the valued object for him in terms of his appetencies and needs. Such a conception of value is purely subjective only

if it is interpreted in terms of an individualistic conception of the self. But if we abandon the notion of an absolute, monadic, self, it is not difficult to see how, while there may be purely idiosyncratic values, there are also social values, whose presence or absence is determined by social criteria. This forces on us a distinction which is of paramount importance for the thesis of this paper, the distinction namely between values in isolation and values in system. While psychologically of small import, a satisfactory discussion of *the judgment* of value cannot be carried on without it, for the values in which moralists are interested are almost exclusively those in system. The objectivity which we can claim for the latter rests not only on the fact that they are objective for anyone who finds them to be characters of objects, but also on the fact that they are ruled by criteria valid for the system to which they belong. A value in isolation is one which does not affect nor is affected by other values of the social individual who holds it. One in system is one which affects and is affected by other values; we could also call these social values. An instance of the former is the value I may find in the fragrance of flowers as I go by a flower shop on my way to work. I enjoy it, but the possession of this value is not likely to affect in any noticeable way the rest of my activity. But the largest number of the values of an individual make up a system, in so far as he possesses a more or less well-integrated personality, and are also part of the larger system which obtains in the society of which he is a member; they are social values.

If a system actually obtains it is possible to find by empirical means "the rule" which governs the judgments made within it. *Objectivity is defined in terms of this rule.* For the rule can be stated and verified; and by means of it the single concrete judgment within the system can in turn be verified and corrected. The procedure of verification consists in finding out whether the judgment in question obeys the rule of the system in terms of which it claims validity. Thus when I say that something is good, and I do not mean merely that I like it, in other words when the judgment does not refer to a value in isolation arising out of an idiosyncratic preference, I mean not only that it will

satisfy an isolated interest, but that it will not interfere with the other interests which make up my system, but will rather aid or foster them. But I also mean that other members of the social system with which I identify myself will concur with this judgment. More concisely stated, I mean that in terms of a certain implicit system of values this object will be found good. Such verifications, rough though they must remain in practice, are often made by us, and are implicit in all our social relations. When I buy a basket of fruit and send it to a departing friend I expect her to find the fruit valuable. The owner of the store works on the same expectation. If he makes a mistake and for some reason no one wants the fruit, he tries to create an interest in it by advertising. If he fails he has made an error regarding a judgment of fact. Thus with all our other values. In society, in a very rough and unconscious way, the correction of our judgments of value and the transformation of our idiosyncratic preferences into objective social evaluations is always going on by means of moral and aesthetic criticism, as well as by what is vaguely described as "the logic of events," or "the school of experience." To pass a public judgment on a given event or object is to submit this judgment to public verification. When serious discussion arises as to the validity of my judgment we appraise the action in terms of vaguely defined criteria. But discussion does not confine itself to the question as to whether the action conforms to the criteria. The criteria themselves may have to be discovered and defined more clearly; and in the cooperative effort to define them, they are transformed in each individual's mind and shaped into socially available principles of judgment; they are objectified. This goes on constantly and takes place even when the discussion may be strongly tinctured with the malice of gossip. Discussion and criticism average the judgment and objectify the criteria. They perform the same function in value judgments that repeated observations and the open criticism of hypotheses perform in scientific judgments. By means of them "the truth" is arrived at.

These remarks have sought to show how objective relativism avoids the individualistic subjectivism to which Plato reduced it,

without appealing to an objectivity which detaches value from interest. There are two extremes to be avoided: subjectivistic relativism, which makes chaos of the phenomena of valuation, and turns our judgments into nonsense and our values into idiosyncratic whims; and the type of objectivism which detaches value from human interest, for it takes the judgment outside of the sphere of empirical investigation on the basis of vague metaphysical speculations which can not be criticized with precision nor checked within experience.

It is necessary to distinguish the practical predicaments from the theoretical difficulties involved in the objective verification of value judgments. In a modern civilized community the systems of value in which individuals partake well nigh defy the human power of analysis. The complexity and conflicts of modern society are reflected in the individual, and his values must perforce remain inconsistent and vague, and it is often practically impossible, therefore, to discover the criteria which govern them. Were not the social scene split by conflicts of all sorts, our values would be somewhat more congruous, and a closer agreement between our judgments would be the result. But we are split and fractured into small groups, some of which have and some of which lack material locii; we share criss-crossing traditions; different religious, cultural, and political loyalties claim us; we are members of different economic classes, and these are in rapid flux; and within the same nation we are divided by regional differences. Our judgments are chaotic because they are made by men whose interests are vague and confused, inconsistent and unstable. The first and most elementary task in defining the criteria of value must therefore be the practical task of defining the groups or classes to which the individual belongs. The second would be that of finding out the particular criteria which are unconsciously used in each system, and if possible the laws of their change. This is an empirical job, and falls within the province of the sociologist, in the widest acceptation of this term. I do not know of any work on this field that begins to tackle the questions here raised. Nor do I know of other hypotheses, outside the organismic and the marxist, by means

of which we could begin to tackle these jobs. But the organismic hypothesis commits the investigator, it would seem, to neglecting or under-emphasizing the importance of the very divisions within society which it would be necessary to study before the laws of social valuation could be discovered. And the marxist, so far as I know, has not been applied to the task I have in mind, though its application to the study of the dynamics of political power has been one of the major achievements of social theory in the last fifty years. But whatever success the study of these questions may meet with, the point these remarks wish to underscore is the methodological one, namely, that the practical difficulties encountered in verifying by objective empirical means our value judgments can not be used as evidence against the thesis of this paper.[1]

4

It has often been urged that values are not susceptible of scientific treatment because they are irrational, and can not therefore be brought within the scope of law. But if science can not deal with irrational facts there can be no science whatsoever, for all facts are irrational. That bodies pull towards each other as they do, that sulphuric acid reacts on zinc to form a salt and give off hydrogen, that animals want oxygen and musicians want music, that Hitler wants war and civilized men want peace—all facts and all sequences of facts are irrational. Facts are opaque, as Plato noticed, and laws are but the formulation of their invariant recurrencies. Science, however, is not defeated because it has to deal with opaque facts. For its needs are fully satisfied when sequences of events are nicely enough formulated to allow of successful prediction and when its hypotheses point to further discoveries. But the argument can be answered in more concrete terms. It is true that an act of valuation, con-

[1] In his brilliant contribution to the organismic approach, *The Study of Man* (D. Appleton Century Co., 1936), Ralph Linton, while rejecting the marxist hypothesis, is himself frankly unable to account for the factors leading to the particular orientations of cultures, and their specific choice of value systems. It is studies which attempt to give us answers to questions like those Linton was led by his theory to ask but left unanswered, that are here suggested.

sidered by itself, and solely with reference to its psychological aspects, may be said to be irrational, since in its uniqueness it is essentially impulsive and blind. But unless something very esoteric is meant by the word "rational," reflection on the consequences which ensue upon the satisfaction of a given impulse confers rationality upon the decision to fulfill or thwart that impulse. What else is meant by a rational choice than that it has been made upon a wide survey of the price it will cost, the sacrifices it will entail and the satisfactions it will bring? An impulse can be studied empirically in respect to the conditions which give rise to it, and to the consequences entailed by its fulfillment. This can be denied on the assumption of the utter spontaneity of the will. No criteria of judgment can be discovered empirically to govern a will which by hypothesis is not determined by empirical factors. The hypothesis however can be disproved since it can be empirically shown that men's choices can and in fact actually are determined by empirical factors—factors depended on daily by men to carry on the educational process.

But the assertion that values are irrational may be intended to point to the fact that value theory can not compel the individual to an act of valuation. With certain reservations to be discussed below this is true. No one, not even a philosopher, can argue a man into or out of wanting something on theoretical grounds. All he can do by way of argument is to show that the existence of a particular want is practically incompatible with other values held by the person to whom the argument is addressed. There are all sorts of ways to alter an impulse in one's self and in another. But the statement "You should want this," or "You should not want that," if meant in any but a conditional sense, is meaningless. But true as this is, it does not render value science impossible. If the act of valuation can not be compelled by value science, neither can any other fact be compelled by any other science. Science does not compel nature; it humbly seeks to discover its habits. We may however compel other men to change their values, but that is because we possess the will and power to do so. But we can not claim science on the side of our

own will except as its tool. But this deficiency, if such it be, holds no less of a metaphysical value theory than of a scientific one. The criteria of value discovered by the so-called philosophic method is not "normative" in a different sense than that discovered scientifically. Empirically discovered criteria are found to be operative within specific ranges of human experience and under determinate conditions. They are "normative" for a given system in that they are the principles that have been found to govern the valuation of that system. The difference between them and criteria discovered by means of "metaphysical" speculation lies in the degree of definiteness which the former possess and the latter lack, a lack which seems to be compensated by the saturated tincture of emotive purple which is possessed by the latter and shunned by the former.

This is to say that value science is descriptive, not normative. Its task is the discovery of the laws which actually govern human choices. These laws or rules are "normative" just in the same sense that scientific laws are normative. Therefore the distinction generally made between normative and descriptive sciences is a distinction without a difference; all science is descriptive of laws which are, within a determinate range of application, and so long as the world retains the habits it had when the law was formulated, normative for the phenomena the law refers to. Human activities are governed by principles of valuation in the same way that the fall of an apple is governed by the law of gravity. The normative aspect of the act of valuation is found in the will itself that chooses this way or that; not in the theory, but in the actual urgent aims and needs of social animals. But this is a fact of human psychology. Its analysis, whatever it may turn out to be, can be carried on in psychological terms. And this analysis is capable of exhausting without residue the fact of felt obligations. Metaphysicians may discover by their speculative methods that the psychological factors into which we can analyze the felt sense of obligation—habit, will, impulse, drive, fear or whatever—are connected with the ontological ganglia of Cosmic Being. Nor would an empiricist, if he could find meaning in such arguments, care to deny these findings, since

the sense of obligation is an event taking place within the Cosmos. But since it is a psychological fact, it is possible to explain it satisfactorily by psychology, and psychology is of course an empirical, natural, science.

But although value theory is a descriptive discipline, like all science it enables us to control the matter to which it refers. It does so by clarifying actual existing values in a society, by bringing conflicts and confusions to light, and by thus helping to anticipate more accurately the consequences of practical decisions. It points to the limitations of a system of values, since for obvious reasons a value system involves certain exclusions as well as certain inclusions. It also throws light on the relations between ends and effective means, and on the manner in which means often alter and sometimes utterly defeat ends. And it shows how norms believed to be operative are given lip service only. By performing these functions value theory is influential in bringing about an alteration of the interests afoot. For systems of value may include dominant impulses towards consistency, towards efficiency, towards broadness or massiveness, towards simplicity or asceticism. And clarification may discover conditions which threaten the dominant impulses or interests of the system.

5

The foregoing discussion has sought to show that value judgments are judgments of fact. But to break down completely the dichotomy between fact and value, it is necessary also to show in what sense should judgments of fact be considered judgments of value. This is not difficult to do when the activity of the mind in the knowledge process is acknowledged. Facts are not merely given—in the realistic sense of the term—to an analytic mind, though there undoubtedly is something given in the facts we handle. Facts emerge from an interaction between the mind guided by interests, in terms of techniques shaped by previous successful activity on the one hand, and on the other a world independent of mind in which mind seeks to discover certain determinate traits which it can use for its own purposes. It

follows of course that the facts discovered by mind are not there by themselves. A fact is always relative to the interests and purposes which led the mind to its discovery. "Discovery" is not the most felicitous term to use here, since in one sense the mind is co-maker of the facts which it discovers; but to say that the mind "constructs" its facts would be to invite a worse misunderstanding than the word "discovery" is likely to entail. Now since the mind in its discovery of facts is guided by interest, since discrimination is always purposive, the facts it discovers have always for the discovering mind a value aspect to them. The dichotomy therefore between fact and value must be abandoned on two grounds. Values are facts, since they can be verified, controlled, changed empirically. But facts are always valuable since they are always sought for in response to an interest.[2]

The objections raised against this view come from several quarters. But it is possible to boil them down to this argument: If facts are always a product of discriminative interest, and relative to the conditions and equipment by means of which they were discovered, what happens to the objectivity of truth? We could answer the argument with a counter question; namely, if one is so obviously interested in defending disinterested truth, how can one say that truth so established is free of interest? This however may sound like a verbal answer. But there is another answer, namely, nothing happens to the objectivity of truth if objectivity is properly conceived. The conception of

[2] This can not give any comfort to those who believe that statements of fact and statements of value are transposable. (Cf. "The Transposability of Fact and Value," by Ray Pepley, *The Journal of Philosophy*, Vol. XXXVI, No. 11.) A statement about an aspect of a complex has for its referent the aspect to which it refers, and not all the other aspects of the complex to which the referent may be interrelated, however intimately. Aspects of a complex are in fact interrelated to other aspects, else they would be simples or atoms and not aspects of a complex. A statement about any one aspect may therefore have the relationship of material implication or entailment to statements about other aspects of the same complex. But it is difficult to see how two statements about different referents are transposable into one another because the referents are interrelated aspects of the same complex. As if statements about Peter were transposable into those about Paul, because they are twin brothers, and hence members of the same family: Even if they were identical twins we should have to keep our referents clear and distinct, or we might find ourselves with a libel suit on our hands.

objectivity involved in the assumption that we can by any means whatever get to know facts-by-themselves is rejected by objective relativism. The only facts we ever get to know are those we discover from some standpoint, for some purpose, and which we verify in terms of determinate procedures. The belief that science can grasp events as they are under the form of eternity is false. The judgments of science aim to achieve the generic human standpoint—*aim*, for the progressive self-correctiveness of science has no logical limit. The effort to get at the generic human stand point implies that one of the interests which guides scientific investigation is the interest to overcome the purely idiosyncratic, unshared, the purely individual interests which determine our common sense judgments of fact. When these are eliminated science reaches the generically human perspective from which the facts discovered can be verified by any human being at any time, irrespective of peculiarities due to individual make up, race, or cultural epoch in which the individual's mind was formed. But the facts arrived at have nevertheless the characters they have in terms of the techniques by means of which they were discovered, and have a value aspect to them since they were sought for in answer to specific interests.[3] These remarks can be summed up in one very brief statement, namely, facts are objects of interest, and have therefore a value aspect to them. But to the degree to which they are obtained by the aid of scientific techniques, and are not merely gathered by unaided common sense, they are exclusively discovered in answer to one dominant interest, which is the interest of excluding from the processes of gathering them all interests of a purely idiosyncratic or subjective kind, and thus getting at them from a purely generic standpoint.

University of Wisconsin.

[3] With the conception of objectivity I have dealt more extensively in a paper entitled "A Note on the Question of 'Class Science'," published inVol. I, No. 3, of *The Marxist Quarterly.*

On the Logic of Imperatives[1]

BY

ALBERT HOFSTADTER AND J. C. C. McKINSEY

I

IT IS the purpose of this paper to carry out a partial syntactical analysis of imperatives. Imperatives form a large body of linguistic expressions, appearing, e.g. in mathematical proofs ("Let f(x) be a continuous function!"), laws, moral injunctions, instruction, etc. For analytical purposes we distinguish between two forms of imperatives, the *fiat* and the *directive*. By a directive we mean an imperative which includes an indication of the agent who is to carry it out. For example, "Henry, don't forget to stop at the grocery!" is a directive. By a fiat we mean an imperative which includes no reference to an agent who is to carry it out. For example, "Let there be light!" is a fiat. This is a distinction made in terms of meaning. If, however, proper symbolic devices were introduced for the formalization of imperatives, it could be made in a syntactical manner. Thus we could distinguish between imperatives which possess a certain operator—the directive operator formed by putting a name within square brackets—as "[Henry] (Let it be the case that Henry does not forget to stop at the grocery)!", and imperatives which do not possess this operator, e.g. "Let it be the case that Henry does not forget to stop at the grocery!" In the following we pay attention only to fiats. This, of course, involves a great limitation of subject matter and excludes topics of great interest.

[1] Received November 7, 1938.

A second distinction is made between the *satisfaction* and the *correctness* of imperatives. Roughly, we understand an imperative to be satisfied if what is commanded is the case. Thus the fiat "Let the door be closed!" is satisfied if the door is closed. It will be seen that the satisfaction of an imperative is analogous to the truth of a sentence. The connective symbols which we introduce later may, on their analogy with the truth-functions of the calculus of sentences, be thought of as satisfaction-functions of imperatives. On the other hand, we may think of an imperative as being correct or incorrect, in the sense that what it requires ought, or ought not, to be the case. Some discussion of "correctness" is provided in Section III.

The present treatment of imperatives will take the following form. Language I of R. Carnap's *The Logical Syntax of Language* is extended in scope by certain additions to the primitive symbols and primitive sentences, so as to constitute a combined language of sentences and imperatives. We call the language thus modified, "Language I_C". We believe that analogous changes could be made in Carnap's Language II, so as to transform it also into a combined language of sentences and imperatives; we choose Language I because its structure is sufficiently simple to make our procedure more transparent.

II

The primitive symbols of Language I_C include all the primitive symbols of Language I, together with the following six new primitive symbols: "!", "$-$", "$+$", "$\times$", "$\rightarrow$", and "$>$". The explanation and interpretation of these is as follows:

(1) We regard imperatives as made out of sentences by the application of a certain operator. Thus, for example, the imperative "Let it be the case that the place 3 is blue!" can be thought of as got from the sentence "The place 3 is blue," by applying the operator "Let it be the case that . . .". The mark "!" is introduced to serve as this unary operator, changing a sentence into an imperative, and is to be read "Let it be the case that . . .".

(2) Correlated to an imperative is another imperative which

requires the opposite, as e.g. correlated to the imperative "! every place is blue" is the imperative "! not every place is blue". Given two such correlated imperatives, we call them complementary, and we introduce the symbol "$-$" as a unary operator which changes an imperative into its complement. Thus if C is the first of the above imperatives, then $\overline{C}$ is the second. It will be seen that if an imperative is satisfied, its complement is not satisfied, and vice versa.

(3) The mark "$+$" is a binary operator such that $C_1 + C_2$ is the "sum" of the imperatives C_1 and C_2; if C_1 is the imperative "Let the place 3 be blue!" and C_2 is the imperative "Let the place 4 be red!", then $C_1 + C_2$ is the imperative "Either let the place 3 be blue or let the place 4 be red!". The "$+$" is taken in an inclusive sense, so that $C_1 + C_2$ is satisfied if either C_1 is satisfied or C_2 is satisfied, or both C_1 and C_2 are satisfied.

(4) The symbol "$\times$" is a binary operator such that $C_1 \times C_2$ is the "product" of the imperatives C_1 and C_2; with C_1 and C_2 as above, $C_1 \times C_2$ becomes "Let the place 3 be blue and let the place 4 be red!", and is satisfied when and only when both C_1 and C_2 are satisfied.[2]

(5) The symbol "$\rightarrow$" is a binary operator which makes a qualified or conditional imperative out of a sentence and an imperative. If S_1 is the sentence "The place 2 is blue", and C_1 the imperative "Let the place 3 be blue!", then $S_1 \rightarrow C_1$ is the imperative, "If the place 2 is blue, then let the place 3 be blue!". This last imperative is satisfied when either S_1 is false or C_1 is satisfied. Thus, if S_1 is false, C_1 does not have to be performed; whereas if C_1 is performed, it does not matter to the satisfaction of $S_1 \rightarrow C_1$ whether or not S_1 is true. Consequently we view $S_1 \rightarrow C_1$ as meaning what is meant by $!S_1 + C_1$ (i.e., in the il-

[2] Since this paper was written, Jørgen Jørgenson has remarked (see "Imperatives and logic", Erkenntnis, Band 7, Heft 4, pp. 288–296) that "there seems to be no reason for, indeed hardly any possibility of, constructing a specific 'logic of imperatives' ". We are of the opinion, that Jørgenson made this statement largely because he had not taken account of the fact, that more than a single verb in an imperative can be in the imperative mood. Thus, for example, one often hears conjunctive commands such as: "Close the door, and open the window!" We believe it is reasonable, and possible, to provide formal rules for the manipulation of such complex imperatives.

lustration, "Either let it be the case that the place 2 is not blue, or let the place 3 be blue!").

(6) The application of the five operators thus far described always results in the construction of imperatives. The next symbol, ">", is an operator which constructs sentences out of imperatives. Thus if C_1 and C_2 are imperatives, then $C_1 > C_2$ is the sentence which may be read "C_1 materially includes C_2". This sentence is true either if C_1 is not satisfied or if C_2 is satisfied, and is analogous to the material implication of the sentential calculus. Thus if C_1 is the imperative "Let the place 3 be blue!" and C_2 the imperative "Let the place 4 be red!", then $C_1 > C_2$ is the sentence "Let the place 3 be blue, includes let the place 4 be red." This may be more naturally read, "Either the imperative 'Let it be the case that the place 3 is not blue!' is satisfied, or the imperative 'Let the place 4 be red!' is satisfied." This sentence is true either if C_1 is not satisfied, i.e., if the place 3 is not blue, or if C_2 is satisfied, i.e., if the place 4 is red. It therefore is true when and only when the sentence "If the place 3 is blue, then the place 4 is red," is true.

(7) Besides the new primitive symbols we also use certain primitive symbols of Language I in a new way. In I_C the symbol "=", which in I occurs between numerical expressions and sentences, occurs also between imperatives. If C_1 and C_2 are imperatives, then $C_1 = C_2$ is a sentence. This sentence means, that C_1 is satisfied if and only if C_2 is satisfied. Let C_1 and C_2 be as in the previous illustration. Then $C_1 = C_2$ may be read, "Let the place 3 be blue, equals let the place 4 be red". The "equals" is not a syntactical equality but a material one. Since C_1 is satisfied when and only when the place 3 is blue, and C_2 is satisfied when and only when the place 4 is red, $C_1 = C_2$ is true when and only when the sentence "If the place 3 is blue, then the place 4 is red; and if the place 4 is red, then the place 3 is blue," is true. In Language I_C the limited universal operator, which in Language I can stand only before a sentence, can also stand before an imperative. If C_1, for example, is the imperative "Let the nth place be blue!", then $(n)7(C_1)$ is the imperative "For every $n \leq 7$, let the nth place be blue!". Similarly we have

in Language I_C limited existential operators standing before imperatives. Thus, for example, if C_1 is as above, then $(\exists n)7(C_1)$ is the imperative "For some $n \leq 7$, let the nth place be blue!". (If we made the analogous extension of Language II, we should also have unlimited universal and existential operators standing before imperatives. It is clear how these would be interpreted. If C_1 were the imperative, for example, "Let the nth place be blue!", then (n)C would be the imperative, "Let every place be blue!".)

In Language I_C we use the syntactical designations of Language I, and in addition the following: for imperatives we use the letters "C_1", "C_2", $\cdots$; and in syntactical context we use the symbols "!", "$-$", "$+$", "$\times$", "$\rightarrow$", "$>$", and "$=$" autonymously.

We now come to the formation rules for sentences, imperatives, and numerical expressions in Language I_C. Every sentence of Language I is also a sentence of Language I_C. Every definition of Language I is also a definition of Language I_C, and these are all the definitions of Language I_C; that is to say, we do not, in Language I_C, admit any new definitions—we do not, for example, admit new predicates defined by definitions involving the mark "!". Every numerical expression of Language I is a numerical expression of Language I_C, and these are all the numerical expressions that Language I_C contains.[3] Besides the sentences of Language I, Language I_C contains new sentences, together with imperatives, got by applying the following rules of formation (in addition to the rules of formation of Language I): (1) if S_1 is a sentence, then $!S_1$ is an imperative; (2) if C_1 is an imperative, then $\overline{C}_1$ is an imperative; (3) if C_1 is an imperative, then $(z_1)Z_1(C_1)$ and $(\exists z_1)Z_1(C_1)$ are imperatives; (4) if C_1 and C_2 are imperatives, then $C_1 + C_2$ and $C_1 \times C_2$ are imperatives; (5) if S_1 is a sentence and C_1 is an imperative, then $S_1 \rightarrow C_1$ is an

[3] These restrictions (of the definitions and numerical expressions of Language I_C) are made, not out of any sort of necessity, but just to simplify the subsequent discussion. It would be easy enough to give a material interpretation, for example, of the bounded K-operator standing before a sentence which contained occurrences of "!", but we have felt that the generality to be gained in such a way would hardly be worth the technical complexities thus introduced.

imperative; (6) if C_1 and C_2 are imperatives, then $C_1 > C_2$ and $C_1 = C_2$ are sentences.

In Language I_C, in addition to the primitive sentences of Language I, we take also all sentences of the following forms as primitive sentences:[4]

PSI12 $\overline{!S_1} = !\sim S_1$
PSI13 $!S_1 + !S_2 = !(S_1 \vee S_2)$
PSI14 $!S_1 \times !S_2 = !(S_1 \cdot S_2)$
PSI15 $S_1 \rightarrow C_1 = !\bar{S}_1 + C_1$
PSI16 $(!S_1 > !S_2) \equiv (S_1 \supset S_2)$
PSI17 $(!S_1 = !S_2) \equiv (S_1 \equiv S_2)$
PSI18 $(z_1)Z_1(!S_1) = !(z_1)Z_1(S_1)$
PSI19 $(\exists z_1)Z_1(!S_1) = !(\exists z_1)Z_1(S_1)$
PSI20 $(C_1 = C_2) \cdot (C_2 = C_3) \supset (C_1 = C_3)$
PSI21 $(C_1 = C_2) \supset (C_2 = C_1)$
PSI22 $(C_1 = C_2) \supset (\bar{C}_1 = \bar{C}_2)$
PSI23 $(C_1 = C_2) \cdot (C_3 = C_4) \supset (C_1 + C_3 = C_2 + C_4)$
PSI24 $(C_1 = C_2) \cdot (C_3 = C_4) \supset (C_1 \times C_3 = C_2 \times C_4)$
PSI25 $(C_1 = C_2) \cdot (C_3 = C_4) \supset (C_1 > C_3 \equiv C_2 > C_4)$
PSI26 $(C_1 = C_2) \supset [(z_1)Z_1(C_1) = (z_1)Z_1(C_2)]$
PSI27 $(C_1 = C_2) \supset [(\exists z_1)Z_1(C_1) = (\exists z_1)Z_1(C_2)]$

The definitions, for Language I_C, of the syntactical notions "sentence S_1 is derivable from sentences $S_2, \cdots, S_n$", and "sentence S_1 is provable" are the same as the corresponding definitions for Language I. Similarly, the terms "refutable", "irresoluble", "analytic", "contradictory", "synthetic", and "consequence of", when denoting properties of, or relations between, sentences. Analogous terms will later be defined for imperatives.

It is now easily seen, that every provable sentence of Language I, is also a provable sentence of Language I_C. It is also easy to show, that the elementary syntactical theorems of Language I hold also for Language I_C. On the basis of these theorems, and by making use of some special theorems regarding imperatives (got from the new primitive sentences) it is then possible to demonstrate, by induction, the following important theorem.

[4] We have not investigated the question, whether these primitive sentences could be weakened, or reduced in number.

Theorem I. If S_1 is any sentence of Language I_C, then there exists a sentence S_2, of Language I, such that $S_1 \equiv S_2$ is provable in Language I_C. If C_1 is any imperative of Language I_C, then there exists a sentence S_1, of Language I, such that $C_1 = !S$ is provable in Language I_C.

This theorem amounts to saying that all imperative-connectives can always be eliminated from a sentence, and that all imperative-connectives except for one "!" symbol can always be eliminated from an imperative (the recursive proof of the theorem leads to a constructive method for performing the eliminations). Moreover, it is easily shown that if $C_1 = !S_1$ is provable and $C_1 = !S_2$ is also provable, then $S_1 \equiv S_2$ is provable, so that S_1 is unique to within derivability. This fact provides a justification for the definitions given in the next paragraph.

Suppose that $C_1 = !S_1$ is provable. Then we call C_1 *provable*, *refutable*, *irresoluble*, *analytic*, *contradictory*, or *synthetic*, according as S_1 is provable, refutable, irresoluble, analytic, contradictory, or synthetic, respectively. Suppose that $C_1 = !S_1$ and $C_2 = !S_2$ are provable. Then we call C_2 *derivable from* C_1, if S_2 is derivable from S_1, and C_2 a consequence of C_1, if S_2 is a consequence of S_1; if C_2 is a consequence of C_1, and C_1 a consequence of C_2, then C_1 and C_2 are said to be *equipollent*. The relation of consequence as applied to classes of imperatives may be defined in a similar way.

In this way we can extend all the various syntactical concepts which are defined for sentences, so that they cover imperatives as well.[5]

Illustrations of the above defined syntactical properties and relations of imperatives will perhaps be useful.

(1) Let "R" mean red. Then "$R(3) \supset R(3)$" is provable. But also "$(R(3) \rightarrow !R(3)) = !(R(3) \supset R(3))$" is provable. Thus "$R(3) \rightarrow !R(3)$" is a provable imperative. Since "$R(3) \supset R(3)$" is analytic as well, the imperative "$R(3) \rightarrow !R(3)$" is also analytic.

[5] If we had chosen to add imperatives to a different language than Language I, we should have different syntactical terms at hand. If, for example, the initial syntax contained P-terms, we could define the corresponding P-terms for commands.

(2) The sentence "R(3). $\sim$ R(3)" is refutable and contradictory. Since "(!R(3) $\times$!R(3)) = !(R(3). $\sim$ R(3))" is provable, the imperative "!R(3) $\times$!R(3)" is also refutable and contradictory.

(3) Let G_I be the Gödel sentence for Language I (cf. Carnap, The Logical Syntax of Language, p. 134). G_I is analytic but irresoluble in I. Thus also !(G_I) is analytic but irresoluble in I_C.

(4) "R(3)" is a synthetic sentence. Thus "!R(3)" is a synthetic imperative.

(5) The sentence "R(x)" is a consequence of the class of sentences {"R(0)", "R(1)", "R(2)", $\cdots$}. Thus the imperative "!R(x)" is a consequence of the class of imperatives {"!R(0)", !R(1)", "!R(2)", $\cdots$}.

(6) The sentence "R(2)" is derivable from the sentence "$\sim$ R(2) $\supset$ R(2)". Thus the imperative "!R(2)" is derivable from the imperative "!($\sim$ R(2) $\supset$ R(2)" or from the imperative "$\sim$ R(2) $\rightarrow$!R(2)".

III

The results of the previous section are in a sense trivial; for the correlation of the syntax of imperatives and the syntax of sentences becomes so close that nothing essentially new is said. (It may however be that this is a consequence of restricting consideration to fiats alone.) It is nevertheless useful to recognize triviality for what it is and where it appears; the fact that such-and-such a thing is trivial, is not in general itself trivial. In particular, it may perhaps be felt that the above theorem, since it makes every sentence involving the mark "!" equipollent to a sentence not involving this mark, shows that the introduction of the "!" is superfluous. We are inclined to believe that this result, far from being a defect in our theory, is rather a recommendation. We feel that it would be desirable, when introducing the mark "!" into almost any sort of language, to assume sufficiently strong primitive sentences and rules, in order to be able to prove such a theorem as ours[6]—about the only exception

[6] Since this paper was written, Karl Menger has published a paper ("A logic of the doubtful. On optative and imperative logic," Reports of a Mathematical Colloquium,

to this would be, if the language in question contained primitive syntactical or semantic predicates, which took sentences or commands as arguments.

There are some points of interest concerning imperatives which are worth considering. First, it would appear as though questions may be analyzed as imperatives. Thus the question "What is the color of the object a?" may be analyzed as "Give a true sentence whose predicate is a color-predicate and whose argument is 'a'!" A question, according to this kind of analysis, is a command to give a true sentence. It involves semantic notions, referring both to language and its subject-matter.

Secondly, the present analysis of commands makes possible a sharper formulation of problems of evaluation as they are considered in ethics and social theory. For example, ethical philosophers have been much concerned with the problem as to whether value judgments can be scientifically established. So far as the *formal* (or syntactical) aspects of this question are concerned, the syntax of commands may be of aid in establishing the conditions of its answer.

The term "value judgment" does not have a traditionally settled meaning. It is frequently difficult to tell whether some, all, or no value judgments are considered to be in the indicative or the imperative mood. Insofar as they are considered to be in the indicative, the question as to their scientific determination would seem to hinge about the question as to whether, e.g., such terms as "good" and "right" are used in a manner which allows empirical confirmation or disconfirmation of sentences in which they appear. With this aspect of the problem we are not at present concerned. The theory of commands be-

---

Second Series, Issue 1, pp. 53–64) in which he maintains that there ought not to be so close a correlation between imperatives and declarative sentences. He also seems to believe, that a three-valued logic is necessary, in order to give an adequate representation of imperatives. We do not feel that this is the case, since we believe that the classification of imperatives into analytic, contradictory, and synthetic satisfies much the same purpose.

Our treatment differs further from Menger's, in that Menger treats an imperative as a certain sort of declarative sentence, whereas we treat imperatives as distinct from declarative sentences.

comes relevant when value judgments are understood to be in the imperative mood. Here the problem might be stated somewhat as follows: can a command be determined to be correct (justified, worthy of being obeyed, right) by scientific means?

As thus stated, however, the problem remains quite vague. No reasonably clear definition has been provided for such terms as "correct", "justified", etc. Indeed it would appear that a reasonable definition of any of these terms would already include, implicitly or explicitly, a specification of operations whereby its applicability would be determined. Consequently, were such a definition provided—say for "correct"—all that would remain would be to investigate whether those operations were scientific operations.

But what means or operations are considered to be "scientific" is neither always clear nor always agreed upon in the discussions of ethical philosophers. Thus even if some definition were provided for "correct" which allowed determination of the correctness of a command by carrying out specified operations, it is not at all certain that this would be accepted as a reasonable definition by all ethical philosophers.

Of the many senses which may be given to the problem as stated above, two may be considered here.

First, it may be taken to be asking whether commands or imperatives can be deduced from the indicative sentences of science alone—whether "values" can be established by "facts". This is a syntactical question, and can receive a definite answer only in terms of the syntax of some language in which imperatives appear. If the language is English, it is very difficult to say what the answer would be. Many people speak English in such a way as to indicate that no chain in which the initial expressions are sentences and only the final expression is an imperative would be recognized by them as constituting a proof or deduction, whereas others speak in different ways. It is likely that if a philosopher who recognizes the sentence "whatever is, is right" to be true—and perhaps also the sentence "the real is rational and the rational is real"—were to attempt to formulate a language containing imperatives, he would have as part of his

definition of "derivable" (or "consequence of") something like the following: if $C_1 = !S_1$ is analytic, then C_1 is derivable from (or a consequence of) S_1. He would have had a purpose in so defining "derivable". In the formulation of commands which we have given, we do not include this in our definitions; we see no important purpose which would be served by including it, and indeed we think that some things that some people do are unjustified.

Secondly, the problem may be interpreted as asking whether the method of forming and testing hypotheses which is used in the usual sciences can be applied in the case of imperatives, and, if it can, whether there could result the construction of a body of warrantable imperative conclusions, intersubjectively agreed to and capable of continuous improvement as far as adequacy and confirmation are concerned, as occurs in the usual sciences.

In order to indicate some points involved in the answer to this question, consider a formulation of a language containing imperatives, somewhat richer than Language I, so that the ordinary imperatives of English find a representation in it, but which is otherwise quite analogous to the present formulation. Suppose this language to be used in the attempt to set up a "scientific" body of imperatives. (1) Corresponding to problems as to what is the case, problems answerable by indicative sentences, there would be problems as to what ought to be the case, what ought to be done, answerable by imperatives. (2) As elements of a situation are selected as relevant to the solution of a problem of the first type, so elements—whether the same or different—are selected as relevant to the solution of a problem of the second type. (3) Corresponding to hypotheses advanced toward solution of the first type of problem, there would be imperative hypotheses advanced toward solution of the second type of problem. (4) Deductive development of indicative hypotheses would have its correlate in deductive development of imperative hypotheses. In the latter case, however, this development would be mediated by the deductive elaboration of indicatives, since "derivable from" and "consequence of" are defined in terms of the indicative components of imperatives.

And here, it would be possible (by so defining the syntactical terms) to have procedures in which the premises are not solely imperatives, but also indicatives. For example, from the imperative $!S_1$ and the indicative $S_1 \supset S_2$ we could then derive the imperative $!S_2$. In any case, the premises would include imperatives and the conclusion would be an imperative. In this way, predictions could be made as to imperatives which would have to be obeyed if the original imperative hypothesis were obeyed. (5) The close correspondence of the procedures however seems to cease when the final questions of confirmation and disconfirmation arise. In the case of the indicative sentences of science there are available data, basis-sentences, protocols, observation-sentences (whatever they may be called) upon which intersubjective agreement is forthcoming. Indeed, the procedure of empirical science allows only such sentences to be used for purposes of confirmation and disconfirmation. In the case of imperatives, however, there does not seem to be a similar body of basis-imperatives. Where an imperative concerning some situation is advanced, and people in general are apprised of its imperative consequences, they do not always agree as to whether those consequences are acceptable in whole, in part, or not at all. And the same phenomenon seems to appear even when those imperative consequences are themselves put to test in the same manner as the original imperative hypothesis. It would seem that it is this state of affairs to which reference is made when it is said that there is a relativity of value-judgments. The answer to the second interpretation of the problem concerning the possibility of a scientific establishment of value-judgments would thus be: (a) a method analogous to that employed in the usual sciences may be applied in the case of imperatives, but (b) there is no reason to believe that intersubjectively confirmable conclusions will result, unless at some time we and our society should all be so constituted, that we should find ourselves in substantial agreement on these matters.

New York University.

The Philosophical Implications of Some Theories of Emotion

BY

M. C. NAHM

AN EXAMINATION of philosophical conclusions and psychological experimentation upon the nature of the emotions raises numerous complex and controversial problems. The terms employed, *viz.* "the life of feeling", "instinct", "imagination" and "emotion" are integral to epistemology, ethics and aesthetics. In epistemology, the teleological aspect of the emotions is of importance. In ethics, the Stoics gave impetus to the demand that the emotions be controlled, a demand that reached its culmination in the Kantian formalism. In aesthetics, the acceptance of the "sensuous medium" as the material of art has implied "feeling" as its subjective counterpart. Yet, the full implications may not be drawn in these particular fields until analysis of the emotions themselves may relate these terms and give them precise and systematic definition.[1]

[1] Since this article was written, K. B. Lashley's *The Thalamus and Emotion* has appeared (*Psychological Review*, Jan., 1938). I regret that its conclusions were not available for inclusion here. It is of interest to note, however, that Professor Lashley examines critically the view that there is a "peculiar quality of emotion" (*p.* 44) and that he casts doubt upon the validity of the evidence offered in support of the thalamic theory. Professor Lashley concludes his examination of the thalamic theory in these words: ". . . the only part of the thalamic theory of emotion which has factual support is the localization

In this analysis, it will be maintained that a sound point of departure for an examination of the emotions lies in the view propounded by William James. It will be maintained, in general, that the emotions are constituted of observable behavior, the limits of which are 'instinct' and 'mood', the behavior varying directly in intensity from overt action to peripheral reaction and inversely with the alternative modes of action implied in the organism's 'images' (*i.e.* stimuli effective for it), implying epistemologically the operation of an inherited *appercepzionsmass* and necessitating for complete explanation ateleological and teleological definition.

On this view, complex bodily behavior related to end or purpose replaces the treatment of the emotions as complex modes of simple sensation.[2] From James' view and from the augmented knowledge derived from the research and controversy of the past half century, the postulate given above will be amplified and defended.

THE COMPONENTS OF EMOTION AND THE METHOD OF CLASSIFICATION

The strength of James' position lies in the implication that the emotions may be examined as observable bodily behavior: 'A purely disembodied human emotion is a nonentity, . . . for *us*, emotion dissociated from all bodily feeling is inconceivable.'[3]

---

of motor centers for emotional expression within the hypo-thalamus. It seems certain that these motor centers do not contribute directly to other aspects of emotion and there is no evidence for the existence of other affective or emotional centers. . . ." (*p.* 60)

[2] This abstraction is that of Locke and the school of "sensationalists".

[3] The divergences of James' statements from those of Lange are considered in this analysis to be critical. Therefore, no attempt is made to defend the "James-Lange theory". James' theory has alliances as old as Aristotle's hypothesis of the soul as form or entelechy of the body (always excepting the Aristotelian "intelligible soul") and in it are expressed views apparently as divergent as those of Descartes and Darwin. *Cf. de Anima*, 6 *ff.*, 211 *ff.*; Darwin, *The Expression of the Emotions*; and Descartes' *Meditations*, 6, and *The Passions of the Soul.* For historical anticipations of the James-Lange theory, *vide* E. B. Titchener, *An Historical Note on the James-Lange Theory of Emotion*, *Amer. Journal of Psych.*, 1914 (25), *pp.* 427 *et seq.*

James couched his theory in forceful language. He wrote[4]:

> Common-sense says, we lose our fortune, are sorry and weep; we meet a bear, are frightened and run; we are insulted by a rival, are angry and strike. The hypothesis here to be defended says that this order of sequence is incorrect, that the one mental state is not immediately induced by the other, that the bodily manifestations must first be interposed between, and that the more rational statement is that we feel sorry because we cry, angry because we strike, afraid because we tremble, and not that we cry, strike, or tremble because we are sorry, angry, or fearful as the case may be.

To this statement, which James called 'crude', may be applied the term 'gross description'. It is clear, for example, that James held weeping to be a far more complicated matter than the mere appearance of tears: 'the bodily affections characteristic—of any one standard emotion[s]', he said, 'are infinitely numerous and subtle'.[5]

For the purposes of exposition, three propositions offered by James may be said to sum up the significant portions of his theory: 1. '*the bodily changes follow directly the perception of the exciting fact, and* [*that*] *our feeling of the same changes as they occur* is *the emotion*'.[6]

2. 'As soon as an object has become . . . familiar and—suggestive, its emotional consequences, . . . , must start rather from

[4] *Principles of Psychology*, vol. 2, *pp.* 449–50.

[5] The great complexity which James attributed to the bodily reaction may be gathered from the following: 'The researches of Mosso with the plethysymograph have shown that not only the heart, but the entire circulatory system, forms a sort of sounding-board, which every change of our consciousness, however slight, may make reverberate. Hardly a sensation comes to us without sending waves of alternate constriction and dilatation down the arteries of our arms. The blood-vessels of the abdomen act reciprocally with those of the more outward parts. The bladder and bowels, the glands of the mouth, throat, and skin, and the liver, are known to be affected gravely in certain severe emotions and are unquestionably affected transiently when the emotions are of a lighter sort. That the heart-beats and the rhythm of breathing play a leading part in all emotions whatsoever, is a matter too notorious for proof. And what is really equally prominent, but less likely to be admitted until special attention is drawn to the fact, is the continuous cooperation of the voluntary muscles in our emotional states. Even when no change of outward attitude is produced, their inward tension alters to suit each varying mood, and is felt as a difference of tone and strain'. (*Mind*, 1884, vol. IX, *pp.* 188–205)

[6] *Principles of Psychology*, vol. 2, *p.* 449.

the total situation which it suggests than from its own naked presence. But whatever be our reaction in the situation, in the last resort it is an instinctive reaction on that one of the elements which strikes us for the time being as most vitally important.'[7] . . . '*Instinctive reactions and emotional expressions thus shade imperceptibly into each other.*'[8]

3. The Darwinian hypothesis: 'Some movements of expression can be accounted for as *weakened repetitions of movements which formerly* (when they were stronger) *were of utility to the subject.*'[9]

Implied in the three propositions and in what has already been written of James' theory are two modes of definition, the one primarily concerned with the physiological and neurological basis for the reaction, the other primarily concerned with the end or purpose subserved by the reaction. These two modes are essential for an examination and criticism of James' position and, indeed, for any general theory of the emotions. They may be given more formal statement as follows:[10] 1. The ateleological or mechanistic hypothesis of emotion attempts a definition of the emotional experience in terms of physiological and/or neurological descriptions of the direct sensory-motor mechanisms (reflex) assumed to be involved, without reference to any end subserved on occasion by the mechanically produced reaction. Emotional experience is thus reduced to *type-of-act.*

2. The teleological or vitalistic hypothesis of emotion attempts a definition of the emotional experience in terms of goal, end or function assumed to be involved, without reference to the *type-of-act.*

1. Ateleological hypotheses

James' description of the type of act called emotional is ambiguous. "We are afraid because we tremble" is not identical with the contention that emotion is the combination in 'consciousness' of the object-(as sensation)-simply-apprehended as idea,

[7] *The Physical Basis of Emotion* (1894) in *Collected Essays, p.* 350.
[8] *The Principles of Psychology*, vol. 2, *p.* 442.
[9] *Op. cit., p.* 478.
[10] *cf.* E. M. Wyman, *On Instinct and Intelligence, pp.* 40, 41.

plus the perception (as complex sensations) of the alterations undergone by "muscle, skin, and viscus".[11] 'We are afraid because we tremble' rightly implies that fear is a way of knowing involved in a total reaction to a situation or inner stimulus.[12]

But 'object-simply-apprehended' and 'object-emotionally-felt' are clearly contrasted by James and the reason is not far to seek. 'Object-simply-apprehended' is clearly analogous to Locke's 'impressions', written upon a passive mind and received from the external world. However, James admits that 'object-simply-apprehended' has its source in 'an object fall(ing) on a sense-organ' or the 'inward excitation' of the sense-organ. This, however, can only mean that there is involved 'sensation' as reaction to stimulus. Stimulus is 'any change in the environment of an excitable tissue which, if sufficiently intense, will excite the tissue *i.e.* will cause it to display its characteristic activity.'[13] Plainly, therefore, 'object-simply-perceived' involves bodily change. It is clear, moreover, that 'object-simply-apprehended' and 'object-emotionally-felt' involve no difference in kind, as regards bodily behavior. 'Simple' perception is not sufficiently simple to preclude bodily reaction to stimulus.[14]

The false dichotomy between 'object-simply-apprehended' and 'object-emotionally-felt' disappears in the alternative view 'we are afraid because we tremble' or even in the statement that the emotions are 'our feelings of the same (bodily) changes as they occur', if the term 'feeling' is not taken to signify unsupported

[11] *Principles of Psychology*, vol. 2, *p.* 374: "An object falls on a sense-organ, affects a critical part, and is perceived; or else the latter, excited inwardly, gives rise to an idea of the same object. Quick as a flash, the reflex currents pass down through their preordained channels, alter the condition of muscle, skin, and viscus; and these alterations, perceived, like the original object, in as many portions of the cortex, combine with it in consciousness and transform it from an object-simply-apprehended into an object-emotionally-felt."

[12] Concerning James' view of 'consciousness', the relevant distinction between experience and reflection, and the clarification of ambiguities found in James' epistemology, *vide* E. A. Singer, Jr., *Mind as Behavior*, *pp.* 6–9, 174–176, 182.

[13] Adrian, E. D., *The Physical Basis of Sensation*, *p.* 18.

[14] The problem is discussed in Grace A. de Laguna's *Emotion and Perception from the Behaviorist Standpoint*, *pp.* 412 *et seq.*, *The Psychological Review*, vol. 26, no. 6, November, 1919.

introspective analysis. For if either of these alternative statements be accepted, the distinction between 'object-simply-apprehended' and 'object-emotionally-felt' is based no longer upon the forced exclusion of bodily reaction in perception but upon observable differences in bodily reaction in the perception of ordinary situations and the perception of emotional situations.[15] The adjustments of the body which constitute at least a portion of ordinary perception are, for the most part, subliminal for the perceiver. In the perception called emotional, the reaction of the perceiver passes the threshold for the experiencing individual, either through rapidity or extent of change. 'Object-simply-apprehended' is distinguished from 'object-emotionally-perceived' by what, in Singer's phrase, *does not* affect the experiencing individual.[16]

Perception is not the collection of simple sensations. It is, rather, the adjustment or reaction of the organism to stimuli. In those instances in which perception is called emotion, the adjustment, as James clearly saw, was a typical reaction which because of the intensiveness or extensiveness of the muscular, visceral and glandular change becomes perceptible for the perceiver. As perception, it appears for the observer as the pallor of the skin, the sweating, the capillary action of the hair, the trembling and palpitation, the clenching of the hands, the diffusion of blood and the dilation of the nostrils. A discontinuity in the external world—or one supposed to exist by the perceiver—calls forth emergency action, the magnitude of which causes the reaction noticeably to become a portion of the total perceptual situation.

The complexity of the emotional reaction is the complexity of an organism reacting as a whole in a preadjusted way to its environment. For, as James saw, the typical reactions are allied to the instincts in that the response occurs without individual training or discipline, is to some degree beyond the control of the individual, and is common to a species or to a sex within that species. As such, both instinct and emotion are best under-

[15] *Vide* E. A. Singer, *op. cit., p. 93 et seq., On Sensibility.*

[16] *ibid., p. 95.*

stood as a portion of the *appercepzionsmass* through which 'every living thing is but a bundle of predispositions to react in particular ways upon the contact of particular features of the environment.'[17]

Nevertheless, despite this epistemological similarity, emotions and instincts may be differentiated as *types-of-act*. The most important differentiation lies in the implications of James' statement that 'emotional reaction usually terminates in the subject's own body, whilst the instinctive reaction is apt to go farther and enter into practical relations with the exciting object.'[18] As will be seen, this differentiation concerns the phenomenon of 'emotional shock'—a checking of the immediate response to a stimulus, ordinarily calling forth instinctive behavior, in such manner that the bodily reaction is displayed not in the direct relation of the organism to the stimulating object but within the body of the organism itself. To the observer, the reaction appears to be abortive and has been termed 'attitude' rather than 'action'.[19]

As has been suggested, James' theory avoids the pitfall of vague feeling and disembodied emotion. As a theory of emotions constituted of observable behavior, its strength was early seen to lie in the possibilities it afforded for experimental verification. James labored under no illusion that the *data* descriptive of the *type-of-act* called emotional was complete. He felt that 'not even a Darwin has exhaustively enumerated *all* the bodily affections characteristic of any one of the standard emotions.'[20] This, in the light of the importance he laid upon Mosso's experiments and upon the readiness to evaluate circulatory, neural, and glandular phenomena in terms of his theory makes it necessary to turn to recent research for evidence in support or in criticism of James' view.

The examination of that portion of James' theory of the emotions as observable ateleological behavior will fall into two cate-

[17] *cf.* E. von Hartmann, *Philosophie des Unbewussten*, ch. X, *p.* 302.

[18] *Prin. of Psych.*, *p.* 442.

[19] *cf.* J. R. Angell, *Psychology*, *p.* 370; W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage*, (1929) *p.* 228; John Dewey, *The Theory of Emotions* (*Psychological Review*, Nov. 1894, vol. 1, no. 6, *pp.* 18–19, 28, and Jan. vol. 2, no. 1, *p.* 595); and James, *Collected Essays*, *pp.* 350–356.

[20] *What Is an Emotion?*, in *Collected Essays and Reviews*, *pp.* 250–1.

gories: 1) The physiological and neurological research which denies the importance of peripheral bodily behavior and substitutes for this dependence upon a central organ. It implies differentiations of 'quality' in emotion and accepts a teleological explanation of emotional behavior. Emphasis will be laid upon the theses of Sherrington and Cannon. 2) The physiological and neurological research which accepts, in the main, James' view of the 'coarser emotions' on ateleological grounds but denies teleological significance to emotional behavior. Emphasis will be placed upon the results recorded by the French psychologists, Dumas, Piéron and Barat, whose work appears in Dumas' *Traité de Psychologie*.

At first sight, Cannon's experimental results appear to be singularly important in support of James' theory of the emotions.[21] For Cannon, although he does not distinguish between instinct and emotion, says of 'Fear, rage, pain and pangs of hunger' that they 'are all primitive experiences in which human beings share with the lower animals'.[22] Moreover, the responses noted are 'typical organic responses—evoked through inherent automatisms'.[23] Specifically, Cannon holds that the bodily changes which occur in intense emotional states result from sympathetic discharge[24] *i.e.* result from the operation of the sympathetic nervous system. The most important of the bodily changes, in addition to snarling, erection of hair, *etc.*, observed in the conduct of experiments upon cats restrained in 'cradles' and confronted by barking dogs, is the measurable increase in the secretion of adrenin and in the sugar in the blood.[25] In terms of James' theory, the evidence of glandular reaction would be an important addition to the research of Mosso, would con-

[21] *cf. Bodily Changes*, particularly pp. 2–3.

[22] *Bodily Changes* vii. But *cf.* denial of this in *Again the James-Lange and the Thalamic Theories of Emotion* (*Psychological Review*, July, 1931); *cf. op. cit., p.* 193, *p.* 194–5 and *p.* 243.

[23] *op. cit., p.* 194 and *cf. p.* 243.

[24] *The Interrelations of Emotions as Suggested by Recent Psychological Researches*, (*American Journal of Psychology*, 1914) *p.* 275.

[25] *The James-Lange Theory of Emotions: A Critical Examination and an Alternative Theory, Washburn Commemorative Volume* (*American Journal of Psychology*, 1927), *p.* 114.

stitute a portion of the totality of bodily change and would *be*, along with the other reactions, (from an ateleological point of view), the emotion. But Cannon specifically refuses to accept James' theory in the interpretation of his results.[26] He holds, rather, that

'the neuro-muscular and neuro-visceral arrangement for the display of rage has its central control congenitally organized in or near a phylogenetically ancient part of the brain, the optic thalamus. The thalamus is not like the cerebral cortex in being a region where new relations with the outer world are registered and old relations are modified; it more nearly resembles the spinal cord—a region under superior dominance, where afferent impulses are received, regrouped, and redistributed either to the higher levels, or to neighboring motor neurons which promptly discharge to effector organs in stereotyped reaction patterns. The typical postures and attitudes which result from action of the thalamus are more complicated that those produced by spinal reflexes but are not essentially different. The physiological organization which establishes the reflex figure of rage I have detailed because it may serve as a prototype for other primitive emotional responses.

In support of his thesis that the thalamus is the center and point of distribution for the emotions, Cannon makes apparent two important factors: In the first place, there is the experimental *data*, ultimately derived from Sherrington's experiments and, in the second place, there is the inference which Cannon draws and which results in his differentiation of *emotional behavior* and *emotional experience*. We shall treat these in order.

Reference has been made to the experiments conducted by Cannon which indicated that accompanying the emotional reaction to stimuli (or constituting that reaction) were augmented glandular activities which introduced extraordinary quantities of adrenin and sugar into the blood. Cannon argues, therefore, that if adrenin is present in increased quantities during emotional disturbance, injections of adrenin should produce similar bodily behavior and, in human beings, offer evidence of a consequent 'feeling' of rage, *etc.* In such instances, Cannon reports that although the bodily reaction was present, to the subject there was

[26] In *Feelings and Emotions, The Wittemberg Symposium, p.* 263.

apparent little feeling of 'real emotion'. Similarly, Cannon argues, patients with fever undergo bodily disturbances similar to those observed in emotional reaction, although there is no similarity in the 'experience'.[27]

The impetus for this experimentation was derived from Sherrington's experiments,[28] and while these were directed primarily against the hypothesis that 'the psychological process of emotion is secondary to a discharge of nervous impulses into the vascular and visceral organs of the body suddenly excited by certain peculiar stimuli and depend(ent) upon the reaction of those organs'[29]—the results tended to cast doubt upon the importance of the bodily behavior. Sherrington severed the spinal cords in the lower cervical region of young dogs:[30]

> To study emotion in a lower animal is not altogether easy, even in a dog. But if reliance be placed on the signs that are usually taken to signify pleasure, anger, fear, disgust, then these animals showed them as unmistakeably after as prior to the transection of the cervical spinal cord.

Sherrington claimed less for the results of his experimentation than did those who followed his method. Although he found that James, Sergi and Lange had expounded theories of the emotions which had in common characteristics cited above,[31] he realized that his results particularly cast doubt upon Lange's emphasis upon the vasomotor theory of the production of emotions.[32] But it is important to remember that Sherrington believed his results to be incomplete with reference to James' version of the theory[33] because James did not consider the vaso-

[27] *Bodily Changes*, *p.* 351 *et seq.* and *op. cit.*, *passim.*

[28] *The Integrative Action of the Nervous System*, Lecture vii, *pp.* 235 *et seq.* and *Experiments on the Value of the Vascular and Visceral Factors for the Generation of Emotions*, *Proceedings Royal Society of London*, Vol. 66, May, 1900.

[29] *Experiments*, *p.* 402.

[30] *op. cit. pp.* 392–3.

[31] *Experiments*, *pp.* 390 *et seq.*

[32] *Experiments*, *p.* 402: "A vasomotor theory of the production of emotions seems at any rate rendered quite untenable."

[33] He says that James' 'suggestive chapters led to the above attempt at examination of his theory, an examination the incompleteness of which I wish to unreservedly acknowledge.' *op. cit. p.* 403.

motor reaction primary to all the other organic and visceral disturbances, 'carrying in their train the psychological appanage of emotion; that James includes other 'motor' sensations and centripetal impulses from musculature than visceral and vascular among those which causally contribute to emotion; and that James' theory was 'competent' for 'coarser emotions'.'[34]

Underlying some of Sherrington's uneasiness at the incompleteness of the experiments is his lack of certainty that animal expression is emotion. But, more specifically, in the experiments, the diaphragm of the animals remained sentient and the facial muscles were unaffected. Sherrington concluded the experiment did *not* obviate those factors in James' theory involving[35] 'centripetal impulses from musculature other than visceral and vascular among those which causally contribute to emotion'.

Sherrington's results actually weigh against the preponderance of any one of the bodily reactions in the emotional experience. Cannon's amplification of Sherrington's experiment has a more ambitious goal. Concluding from the experiments upon ductless glands that the emotional center is the optic-thalamus *i.e.* that the most important factor is not 'peripheral'—he inferred that the emotional response is a *pattern reaction* in which impulses flash through peculiarly co-operating neuron groups of the central nervous system.[36] To support the view, to which credence had been given by the experiments with adrenin, Cannon investigated the reports of pathological cases in which individuals afflicted by paralysis gave evidence of emotional experience despite the paralysis of muscles ordinarily involved in the expression of emotion.[37] The value of the instances cited is doubtful as a refutation of James' hypothesis. There is no evidence offered of any attempt to check minute alterations in the bodily behavior. All that is noted is the absence of 'gross' alterations. Moreover, there is overlooked the adaptability of organisms in taking over functions by related organs. But primarily, the difficulty lies in

[34] *The Integrative Action*, *p.* 251.

[35] *Ibid. p.* 259.

[36] *Bodily Changes*, *p.* 228, *p.* 247.

[37] *Again The James-Lange and the Thalamic Theories of Emotion*, *The Psychological Review*, 1931, citing Bell, *The Nervous System of the Human Body*, 1833.

the fact that the evidence is supported only by the unsubstantiated reference to the oral testimony of patients.

In final support of his hypothesis, Cannon argued that there is insufficient differentiation in the reaction of the striped muscles to account for differences in emotional states *e.g.* the differences between fear and rage in which, Cannon reports, the visceral alterations are identical.[38]

So much for the evidence. Cannon infers from it that *emotional behavior* and *emotional experience* must be differentiated.[39] The first step in this differentiation, it will be remembered, resulted from the failure of human beings to report emotional feeling after injection of drugs, although they showed emotional behavior. The second step is the denial that the behavior of animals, which formed the basis for both Sherrington's and Cannon's experimentation, may be termed 'emotional'. The latter position, Cannon does not, however, hold consistently. As has been pointed out, for example, his principal book begins with the hypothesis that 'Fear, rage, pain, and the pangs of hunger are all primitive experiences in which human beings share with the lower animals.' Moreover, Cannon has approached an acceptance of the identification of hunger and muscular contraction.[40]

What foundation has Cannon's demand that 'emotional experience' and 'emotional behavior' be distinguished? The primary differentiation clearly turns upon what is meant by behavior. Equally clearly, Cannon places speech in a category completely separate from the expression of the emotions by muscular and

[38] *Bodily Changes*, *pp.* 351 *et seq.*

[39] *Again The James-Lange and the Thalamic Theories of Emotion*, p. 285: 'Newman, Perkins and Wheeler have failed to discriminate between emotional *behavior* and emotional *experience*. According to the James-Lange theory the two are inseparable—the bodily changes result from impulses from the cortex and report back to the cortex, so that, if the changes occur, the cortex must have received and mediated an affective stimulus or stimulus pattern and would therefore be receptive to reverberations from the periphery. In this view there is no true emotional behavior without emotional experience and, *vice versa*, there is no felt emotion without peripheral changes.—The thalamic theory, on the contrary, admits the possibility of separation—the typical *reactions* have their source in the thalamus, and whether there is an *experience* of emotion or not depends on whether the cortex is present and proper thalamo-cortical connections are intact.'

[40] *Bodily Changes*, *pp.* 290–293; *cf.* particularly *p.* 296, 'Hunger, in other words, is normally the signal that the stomach is contracted for action.'

bodily reaction. The assumption throughout is that introspective analysis, if formulated in words, is more valuable than minute examination of bodily reaction or, indeed, that verbal reports from pathological patients may be accepted without accompanying and corroborating *data*. No one would deny the value of verbal reports or the accuracy of speech and, clearly, the evidence offered by human beings is important in the determination of the nature of emotion. But just as clearly it is only an important portion of the evidence and does but add to the knowledge obtained by other means. As it corroborates bodily expression, it adds evidence for the probability of the conclusions obtained by empirical investigation. That human beings have the capacity for speech adds an important reason for the acceptance of the *data* obtained by an analysis of their behavior. The presumed absence of speech in animals makes the problem of analyzing the particular emotion they undergo more difficult. It does not make it impossible. The experimental method involves the collection and evaluation of all evidence. That information of scientific value may be obtained by scientific analysis of animal behavior has been sufficiently demonstrated by investigators from Darwin to Köhler.[41]

Cannon has thrown light upon the precise functions of the sympathetic nervous system and of the optic thalamus in the emotional response. His investigation of the bodily changes in emotion is important. But it does not follow from his investigation that the optic thalamus adds quality of emotion to 'simple sensation'. Leaving aside the difficulty of 'simple sensation', there is the leap beyond empirical verification in the attribution to the diencephalon of the function of differentiating the types of emotional experience. The hesitation in accepting the hypothesis is not due solely to the reluctance attendant upon abandoning quantitative analysis for qualitative differentiation based upon introspection. Such abandonment would be forced only if the argument took cognizance of all the facts. But certain details upon which Cannon's inference is based require further analysis.

In the first place, Cannon's argument that the striped muscles

[41] The general principle underlying the search for completeness in experimental findings is expressed clearly by Berkeley.

react too slowly to be of importance in emotional responses is not completely substantiated[42] and, granted that it were, it may still be argued that the emotional reaction is posterior to instinctive reaction. Secondly, the statement that no differentiation in the visceral reaction is found in different emotions, *e.g.* fear and rage, is based upon 'gross observation'. Moreover, if more minute observation bears out the inference, Angell has pointed out that one would expect the differentiation of the emotions to lie in the peripheral behavior, with a common *type-of-act* in all emotion.

More important, however, is the naïve conversion of the proposition 'emotion implies typical behavior' (*e.g.* increased glandular reaction, etc.) to the proposition 'typical behavior implies emotion'. The first proposition implies *type-of-act* as necessary but insufficient to the explanation of the emotions. The second proposition ignores the fact that emotions are explicable at least in part by the end served. To argue that James' theory is in error solely because there arises the possibility of producing similarity of ateleological behavior by the injection of adrenin is to overlook the teleological implications in the original hypothesis.

The importance of this fact is to be seen in the very *data* upon which Cannon bases his inference. In the experiments conducted by Marañon, it was found that, in human beings injected with adrenin and displaying, on the whole, 'emotional behavior', 'in exceptional cases of preparatory emotional sensitization the bodily changes may tip the scales towards a true affective disturbance.'[43] Moreover, Cannon admits, while he holds that 'the thalamic neural patterns precede the postural and organic

[42] *Cannon's Theory of Emotion*, Newman, Perkins, and Wheeler, *Psych. Review*, 1930, *p.* 315.

[43] *The James-Lange Theory of Emotion*, *Washburn Comm. Vol.* (*Amer. Jour. of Psych.*), *p.* 114. 'From the evidence adduced by Marañon we may conclude that adrenalin induces in human beings typical bodily changes which are reported as sensations, that in some cases these sensations are reminiscent of previous emotional experiences but do not renew or revive the experiences, that in exceptional cases of preparatory emotional sensitization the bodily changes may tip the scales towards a true affective disturbance. These last cases are exceptional, however, and are not the usual phenomena James and Lange supposed. In normal conditions, the bodily changes, though well marked, do not provoke emotion.'

changes', that 'there is evidence of discharge to the viscera and to the skeletal muscles in typical combinations'.[44]

Now, clearly, the 'preparatory emotional sensitization' involves the only case in which the function of emotion has been considered in these experiments and quite as clearly 'the bodily posture' makes the essential difference. The reason is apparent. Fear and rage and the other emotions are concerned with external situations or imagined (internal) stimuli. The presuppositions of fear and rage are certain situations productive of typical reaction.[45] No analysis of emotion may neglect the *type-of-act*, glandular, postural or skeletal. But equally, no theory of emotion may neglect the end subserved by such reactions or the purpose accomplished or not accomplished. The total bodily behavior in relation to the end subserved may afford us sufficient data to avoid the introduction of 'quality added to simple sensation'.

Cannon's conclusions concerning 'quality' of emotion in contrast to 'emotional behavior' are the more curious in ignoring such teleological factors of emotional behavior, for his own suggestion is that the emotions function as 'preparation for emergency action'.[46] Whether the function suggested is correct or not will be discussed later[47] but granted the teleological hypothesis there is one added factor which may be suggested to account for the 'quality' added to 'simple sensation'. Probably the difference in situation and the minute differences in posture and skeletal adjustment are sufficient for the large degree of differentiation in the emotions themselves. But the actual movement—the process—and its success or failure with reference to the goal or end, the sense of direction accompanying the effort will offer adequate explanation for the differentiation. The 'joy' or 'sorrow', the 'pleasure' or 'pain' accompanying the emotional disturbance are, in all probability, the poles of attainment or failure in process of which the organism undergoes change.

[44] *Again the James-Lange—pp.* 288, 290. *Cf. Bodily Changes, pp.* 356–7.
[45] *Cf.* de Laguna, *op. cit.* and *Cannon's Theory of Emotion, loc. cit.*
[46] Cannon, *Bodily Changes, pp.* 193, 228, 275; *cf. Wittemberg Symposium, p.* 257.
[47] *cf. infra pp.* 482–4.

But leaving aside this particular failure of analysis on the part of Cannon, may not the emotions be completely explained without reference to the end subserved or the goal attained or not attained? May not the glandular, skeletal and visceral behavior be described with sufficient minuteness and adequacy of detail to replace teleological by mechanical definition? Nowhere has the attempt to secure such definition been pursued with more rigor than by the French physiological psychologists whose work has been published in Dumas' *Traité de Psychologie*. Here is combined the traditional Cartesian hypothesis[48] that the life of the passions is the reaction of *automata* to stimulus in terms of the expansion and contraction of nerve and muscle and the 'scientific objectivity' of Pavlow[49] who contrasts the views of the physico-mechanical school to the 'unscientific tendency—of speculation and teleology'.[50]

The search for such 'objectivity' is of particular interest because it accepts, within limits, the validity of the James-Lange hypothesis.[51] But it accepts this hypothesis only on its ateleological side and condemns its teleological aspects as 'finalisme theologique'.[52] The Cartesian phrase, "expansion and contraction" is replaced by the terms 'tonus' and 'hypotonus' but the underlying motive is the same as that which led Descartes to his conclusions upon the automatism of the brute—the explanation of emotions solely in terms of their physiological and neurological

[48] *cf. Med.* 6; *Passions of the Soul; Letter to the Marquis of Newcastle; Letter to Henry More.*

[49] *cf. Conditioned Reflexes, p.* 7 (*Eng. trans.*): "Our starting point has been Descartes' idea of the nervous reflex. This is a genuine scientific conception, since it implies necessity."

[50] *Lectures on Conditioned Reflexes, pp.* 48–50. *Cf.* on Descartes *op. cit.* p. 4.

[51] *Traité, p.* 681. 'Tandis que l'expérimentation physiologique n'arrive pas à infirmer la théorie de Lange-James—'. The further stipulation is made, however, that 'l'observation psychologique et l'observation clinique permettent d'être plus négatifs.' Dumas finds this to be particularly true in the problem of the 'émotions délicates'.

[52] *Loc. cit.; vide pp.* 632–5 for criticism of Darwin and Spencer. It is implied consistently in the *Traité* and, indeed, in many attempts to justify the complete exclusion of teleology, that the conception of 'end' or 'purpose' inevitably involves theology. The argument appears to rest, for the most part, upon the simple statement that 'finalisme theologique' results if any other than mechanical principles are employed but it is obviously directed against the introduction into 'science' of proofs for the existence of God

bases.[53] An illustration of the method employed will be valuable. Barat, writing on the mechanical conditions of different expressions of emotion[54], concludes:

If one is willing to accept our conception of excitation or depression, at the same time making reservations on those parts of our schematization which seem too dogmatic, one may fruitfully turn back to a great number of mimic expressions that Spencer, Darwin and Wundt have explained by their psychological principles and that are in reality the consequence pure and simple of variations of excitation and depression.

We have already pointed out the importance of variations of muscular tone (tonus) in our affective tonality; these same variations do not fail to play a rôle in most emotional expressions, first by themselves and then because they facilitate or impede contractions more or less complicated, reflex, automatic, half or entirely automatic, which are part of the expressions of anger, of joy, of sadness and of fear.

In joy, for example, simple tone can suffice to lift up the head, round out the cheeks, give to the entire countenance the well known expression of satisfaction, and facilitate in that way all the contractions of the voluntary musculature by which the light excitation of joy is

---

based upon the purposive nature of the world. However, simply because phenomena are interpreted teleologically does not necessarily imply a) that all purposes in the universe are completely coördinated or b) that they are coördinated by a deity. In fact, either implication involves an infinite regress which by definition excludes the final term. Kant, in his discussion of the physico-theological proof for the existence of God, has shown with sufficient clarity the fallacy involved. The author of the *Kritik* adds, moreover, the equally important empirical objection to the argument, the fact that "we can never find in experience material sufficient to satisfy such a concept (the idea of God)—We are not acquainted with the whole content of the world, still less do we know how to estimate its magnitude by comparison with all that is possible." But Kant holds with equal clarity that the concept "is never decisively contradicted by any experience" that "it contributes," as a regulative principle, "to the extension of the employment of reason within experience, through the guidance which it yields in the discovery of order and purposiveness." (*K.d.R.V.*, A 621 *et seq.* (arranged)). Kant sums up the argument succinctly in the *K.d.U.*, Sect. 75: "On the question, therefore, whether or not any being acting designedly stands behind what we properly term physical ends, as a world cause, and consequently, as Author of the world, we can pass no objective judgement whatever, be it affirmative or negative."

[53] *op. cit. e.g. p.* 633, Concerning the expression of the emotions: 'It is then indispensable to give a large place to the phenomena of excitation or of neuro-muscular depression in the explanation of emotional expressions; many of these expressions are only phenomena of *tonus*, *hypotonus*, of excitation, of exhaustion, of inhibition. . . .'

[54] *op. cit.*, *pp.* 629 *et seq.*

discharged. There is thus at the basis of the joyous expression, however complicated, however mixed with mental representations it may be, a simple phenomenon of mechanics. . . .

In analyses similar to that quoted above, emotion is reduced to an association 'with an affective discharge of abnormally intense nervous energy'.[55] But it will be noted in the quotation from Barat that the demand for strict objectivity and the complete denial of teleology has already given way to the statement that simple phenomena of mechanics be considered the basis for 'joy'. This would not be denied but it is a statement far different in character from that which requires the elimination of all purposive interpretation. There are, indeed, ample if superficial indications that complete success has not been attained by the ateleologists in their analyses of the emotions without reference to the end subserved. Dumas, for example, in the revised edition of the *Traité*, is content to suggest that 'from the facts and the laws... the strictly mechanistic explanations of expression can go much farther than one might ordinarily think'.[56] And in fact, Dumas holds that 'in the case of very simple emotions, such as passive sadness and active joy, one may stop here'.

But may one stop here? If so, it is only because sufficient teleological explanation is already incorporated into the terms 'sadness' and 'joy' to leave the inquirer satisfied. For, on strictly mechanistic grounds, these terms are not admissible.[57] It has been seen that the mechanical hypothesis involves the

[55] Piéron, *Feelings and Emotions*, *Wittemberg Symposium*, *p.* 285 *et seq.*

[56] *Traité*, *p.* 200. It is of even greater interest to note that between the first and second edition of the *Traité*, the problems raised by purely mechanical explanation have caused such difficulties that Dumas, while still holding that such explanation is adequate for "passive sadness" and "active joy" adds (*loc. cit.*) that the sound conclusion is that one must lay "considerable emphasis on the psycho-reflex and instinctive reactions which are mingled with the reactions of excitation or depression. . . ." He hastens to add, however, "but much less so than did Darwin. . . ."

[57] This inference was clearly stated by Pavlow, *Conditioned Reflexes*, *p.* 4: "It may be hoped that some of the more complex activities of the body, which are made up by a grouping together of the elementary locomotor activities, and which enter into the states referred to in psychological phraseology as "playfulness," "fear," "anger," and so forth, will soon be demonstrated as reflex activities of the sub-cortical parts of the brain." *cf. loc. cit*, *pp.* 6–11.

minute enumeration and description of muscle, nerve and reflex action. Since the description is of distributive factors, the use of the collective term 'joy' has no more significance than if the authors had used the letter 'x' to signify the collection. The question remains: Why, in the mechanical theory are the terms fear, joy, sadness, rage, etc. used? Inevitably, the answer seems to lie in the end these mechanical reactions subserve.

Some attempt, indeed, is made to avoid the consequences of the admission of such terminology. Dumas, for example, argues that the expression of the emotions upon which teleological explanation bases its analysis 'is not true expression either in the psychological or the biological sense of the term'. This is true, it is argued,

> since the subject who experiences the emotion has neither the intention nor the desire to express unless he adds imitation to his emotion. . . . It is the observer . . . who creates the term of expression and gives it a meaning . . . by making a symbol of a spontaneous reaction which was naturally linked to an affective state but which did not have for its purpose the signifying of it.[58]

But, clearly, Dumas may not carry this argument to its ultimate conclusion, since even the mechanistic responses are best analysed by the observer of that behavior. Moreover, if as Dumas insists, it is 'solely the fact of their being visible which has caused these physiological reactions to be considered expressive'[59], it is similarly only because they are perceptible to an observer that the reaction may be treated in any way scientifically.

The case for the mechanist rests entirely on the adequacy of his explanations. And, it is admitted that mechanism is inadequate to the task of explaining the emotions:[60]

> In reality, what prevents our mechanistic explanations from being extended to all the muscular expressions, is that the active expressions of anger and of fear, like the entirely instinctive reactions of defense and aggression, are too complex to be reestablished as variations of the excitation.

[58] *Traité*, ed. 2, *p.* 201.
[59] *ibid.*, p. 201.
[60] *Traité*, ed 2, *p.* 200.

The assumption of the mechanist is, naturally, that the inadequacy of explanation is due to lack of technical refinement in experimentation or to limitations upon the extent of the experiment. But is this really the case? One is reminded that the strongest motive for Kant's search for a basis for the teleological judgment lay in *a priori* necessity, dictated because 'the forms of nature are so manifold' that the concept of purpose is essential to secure unity of empirical principles. It is too often thought that the introduction of teleology signifies the exclusion of mechanical explanation. It has been argued here, however, that the ateleological or mechanical basis for emotional behavior is a necessary but insufficient presupposition of the emotions. The mechanical basis of the emotions may no more be dispensed with than one may dispense with the mechanical principles governing the pull of the trigger on a revolver or similar principles governing the explosion of the shell in the chamber. But just as the murder which results is not wholly explicable in terms of mechanics, so it is argued that emotion is not explicable solely in terms of the mechanical reaction of organism to stimulus. It is not completely explicable because it is not even definable in mechanical terms alone.

This, at its simplest, is what James meant by the 'total situation', involving mechanical and teleological principles. Concerning the adequacy of ateleological defining, Barat, its supporter, concludes 'that in this part of psychology where so many facts still escape us, the greatest error that one can commit in presenting a theory of emotion, would be to consider this theory as sufficiently explicative'. The insufficiency of ateleological explanation is not due, one is tempted to believe, to lack of precision or technique but, rather, to an unwillingness to avail itself of objective facts of another but no less scientific order.

The substantiation of the hypothesis that the emotions are explicable or even definable solely in ateleological or mechanical terms may seem to some to await a new technique or, in Kant's words, 'another Newton'. This is implied in Dumas' statement that certain emotional reactions are "too complex" for mechanical explanation. But the emotions, like the blade of grass which

Kant uses to illustrate his point, come under the category of teleological and purposive. To the suggestion that they may be defined completely in terms ignoring this fact, one may reply in the words of the *Kritik der Urtheilskraft* (Sect. 75):

> It is . . . quite certain that we can never get a sufficient knowledge of organized beings and their inner possibility, much less get an explanation of them, by looking merely to mechanical principles of nature. Indeed, so certain is it, that we may confidently assert that it is absurd for men even to entertain any thought of so doing or to hope that maybe another Newton may some day arise, to make intelligible to us even the genesis of but a blade of grass from natural laws that no design has ordered. Such insight we must absolutely deny to mankind.

THE TELEOLOGY OF EMOTIONS

It has been argued that emotions are wholly explicable neither in terms of the operation of the autonomic nervous system nor of the physiological structure of organisms without reference to the end subserved or the goal sometimes attained by means of the glandular, visceral and peripheral changes. The behavior called emotional must be considered collectively as well as distributively.

One particular phase of the ateleological behavior called emotional lends itself as a starting point for the determination of the end subserved by such behavior. This is the close relation of emotion and instinct as *types-of-act* describable as 'inborn pattern reactions'. In the postulate of the emotions formulated at the beginning of this article, reference was made to the implications of an *appercepzionsmass*, the nervous and somatic presupposition of experience, common to many species and marked off from experience acquired by training, discipline or education of the individual. Epistemologically, instincts and emotions 'shade imperceptibly into each other' with reference to what may be termed 'reproductive imagination'[61] *i.e.* the recognition of a stimulus in a present situation as identical or similar to an image—kinaesthetic, visual or auditory—brought to the experience by the individual, and this without post-natal training.

[61] I have adopted Kant's phrase. *cf. K.d.R.V. pp.* A 118–123 *et seq.*

It was suggested at the outset that instincts and emotions are related in a continuum of observable behavior. Therefore, despite the similarities of the two forms of behavior, so close that the two merge imperceptibly, they may be differentiated not only ateleologically but in terms of end.

The primary ateleological differentiation James held to lie in the fact that 'emotional reaction usually terminates in the subject's own body, whilst the instinctive reaction is apt to go farther and enter into the practical relation with the exciting object.'[62] Taken literally this statement implies a teleological definition of instinct and an ateleological one for emotions. But the essential portion of James' view may be elaborated in Piéron's analysis,[63] summarized in the following excerpt:

> An animal is threatened; he gives a defense reaction, a stereotyped reflex, or a more plastic and better adapted response. This reaction is generally connected with an affective orientation of behavior tending toward flight or aggression. There is no reason for invoking an emotion. But, in addition to the defense reaction, the animal displays processes which are foreign to this reaction and, like it, are aroused by the threatening stimulus. This is the emotional reaction.

Tentatively, the distinction between instinct and emotion is that between overt behavior and peripheral behavior, although both are aroused by the stimulus and are unlearned. Of such emotional nature is the capillary action of the hair, sweating in fear, dilation of the nostrils, *etc.*

In both James' and Piéron's statement, a teleological definition of the instincts has been implied. And that no instinct may be recognised as such without reference to the purpose involved some of the time may be maintained in the face of the considerable controversy which the problem has aroused. For our purposes, a simple illustration of the teleology of the instincts may be given in the often described reaction of the Anthophora. The beetle Sitaris lays its eggs at the entrance of the underground passage of the bee, Anthophora. The larva springs on the male bee,

[62] *Principles of Psychology, p. 442.*
[63] *Feelings and Emotions, Wittemberg Symposium, p.* 286.

remains until the 'nuptial flight', 'transfers to the female bee and waits until the eggs are hatched. It then eats the eggs and is metamorphosed to the insect: Everything happens as if...the Sitaris itself knew that its larva would know all these things'.[64] But more observant experimenters have pointed out that the Sitaris larva becomes attached to any object which passes through the underground passage and that the reaction resembles a simple reflex response to stimulus. But it may be observed that the continuity of the Sitaris as a species is dependent upon the occasional success of the larva in attaching itself to the Anthophora and that the reaction of the Sitaris takes on meaning only when the reaction is viewed in terms of purpose *i.e.* self-preservation, the preservation of the species or, in general, in the control of nature.

Instinct, in the sense of the illustration, may be defined in Bernard's terms[65] as 'an inborn (in the sense of inherited) activity process which has remained intact, that is, which has not been remade through the process of learning or of making new adjustments by means of the substitution of new stimuli or responses for the old which are inherited'. But, Bernard's further view to the contrary[66], it is definable in terms of the end of the control of nature. Some characteristics of this response show its limitations and aid us in securing a knowledge of the teleology of the emotions. It will be seen, for example, that instinct as the lower portion of the continuum, is largely dependent for the successful achievement of its goal upon the presentation in environment of the appropriate stimulus and of no stimulus deleterious to the completion of its purpose. Such deleterious stimuli, *e.g.* the rapid alteration of temperature in liquid surrounding amoebae, introduce the possibility that the highly mechanical reaction to stimuli will be unseasonable and, consequently, an inefficient means to end.[67]

In more highly developed organisms, *i.e.* in organisms not only capable of variation of response but likewise able, because of

[64] *cf.* H. Bergson, *L'Evolution Créatrice*, *pp.* 158–9.
[65] Bernard, *Instinct*, *p.* 84.
[66] *op. cit.*, *cf. pp.* 258–60 and 288 *et seq.*
[67] *cf.* E. A. Singer, Jr., in *Mind as Behavior*, *pp.* 86 *et seq.*

this possible adjustment, to live in environments less constan than water, for example, the presence of other stimuli marking the unseasonable nature of the direct response to the dominant stimulus must be noted by the organism if it is to survive. In this sense, Rousseau wrote that 'The Brute cannot deviate from the rule prescribed to it'.[68] And also in this sense, James wrote that 'whatever be our reaction in the situation, it is in the last resort an instinctive reaction to that one of its elements which strikes us at that moment as the most vitally important.' But as James also suggested, ' "Objects"—the primitive arousers of instinctive reflex movement—' take their place as elements in the 'total situation' in which there are 'other suggestions of which many prompt to movements of an entirely different sort'.[69]

As Kant suggested, there is in emotional experience a checking and a recovery[70]. The variation or checking of the primitive instinctive drive or tendency, seasonable or unseasonable, is dependent upon the 'awareness' in (*i.e.* effectiveness upon) the organism of the alternative stimuli (external or internal) in the total situation. Such 'awareness' is observable in the behavior of the organism but it is, as may be expected, sometimes incorporated in the instinctive action. The total set of stimuli are integrated by the organism in such manner and at such speed that overt behavior follows and the organism is brought into 'practical relation' with the 'object'. In such instances, *e.g.* in the capillary action of the hair, dilation of the nostrils, *etc*, the reaction becomes a portion of general flight and constitutes the instinct 'fear'.[71] Equally well, however, the integration may not take place and the result may be not action but paralysis:[72]

"Thus we have in anger, in fear, in joy, in sadness and in emotional shock, reactions of the mental and physical organism which are in no way implied in the notion of the principal inclination, although

[68] *Discourse on the Origin of Inequality*, Part 1.

[69] *The Physical Basis of Emotion*, in Collected Essays, *p.* 350.

[70] *K.d.U.*, *cf.* Sect. 15.

[71] *Cf.* Bekhterev, *Emotions as Somato-Mimetic Reflexes*, (*Wittemberg Symposium*, *pp.* 274–5).

[72] *Traité de Psychologie* (*ed.* 1923), *p.* 479. *Cf.* Piéron in *Emotions in Animals and Man*, *Wittemberg Symposium*, *pp.* 286, 290; *cf.* Cannon, *Bodily Changes*, *pp.* 228 and 234.

this inclination be the primitive and fundamental condition of the emotional complexus in which it is met, satisfied, unsatisfied, thwarted, paralyzed or excited."

From this there begins to emerge a clear idea of the teleology of the emotions. For, since instinct and emotion both imply glandular reaction and muscular response, emotion may not be defined solely as "preparation for emergency action".[73] There is not only such bodily preparation but also there is implied in emotional reaction the 'awareness' (*i.e.* the effectiveness) of alternative stimuli or alternative modes of action. Epistemologically there is not only as in instinct a reproductive[74] image allying a present stimulus to the *appercepzionsmass* but imagination in the true sense of the word—the presentation by the organism to itself of an alternative stimulus offering possible modes of action. The individual carried over into instinctive behaviour is blind to alternative stimuli.

The view here maintained is best illustrated by reference to the phenomenon known as emotional conflict or emotional shock.[75] In emotional shock the organism's instinctive impulse directed to or from the external stimulus is checked at the limit of the agent's body. A total situation, one element of which demands immediate action, results in glandular, visceral, nervous and peripheral muscular preparation for action. As Angell has suggested[76] when "the reflex and hereditary responses of the organism are inadequate to cope with the demands of the environment", emotion appears. "Whenever there is conflict among the motor impulses called forth by any specific situation", the phenomena called the emotions is present. Angell further suggests that the "significance of emotion as a fact of consciousness would seem, therefore, to be resident in this monitory function, represented by its compelling announcement of needed adjustments, its report of unstable equilibrium".[77]

[73] *cf. supra*, p. 472.
[74] *cf.* note 61.
[75] *cf.* McCurdy, *Psych. of Emotions*, ch. viii, *pp.* 58–65.
[76] *Psychology*, *p.* 323. *cf.* McDougall, *An Outline of Psych. pp.* 325–6.
[77] *Ibid.*, *p.* 323.

Emotion therefore seems to be bifurcated in action. It either assists or hinders some primary tendency or instinct, but clearly in either case it involves the effectiveness of stimuli not apparent to the organism immediately moved by instinct.[78]

It is apparent, then, that in the emotional-instinctive continuum both reactions involve reproductive 'images' (*i.e.* effective stimuli). In instinct, however, the organism's activity is governed by the effectiveness of one productive image,[79] or effective stimulus. In emotion the organism's activity indicates the awareness or effectiveness of more than one productive image or stimulus. In certain instances productive images or stimuli are integrated with the image or stimulus producing the instinctive tendency.

Dewey[80] has characterized the emotions as "attitudes" towards a stimuli-situation and the term is significant of the contrast between the overt action of instinct and the abortive behaviour of emotion. This implies that the attitude—the shrug of the shoulder, the clenching of the fist—was once a useful activity or a primary instinct now inhibited. In their reduced state they may be[81] "useful as supplying a contributory, a reinforcing or checking factor, in some more compulsive activity" and in these varying possibilities "we have all the conditions for high emotional disturbance".

Taking the continuum of emotion and instinct as a whole, we find that the observable behaviour involves a reduction of more

[78] *cf.* R. P. Angier, *The Conflict Theory of Emotion, Washburn Commemorative Volume, p.* 401: "Whenever a series of reactions required by an organism's total 'set' run their course to the consummatory reaction which will bring 'satisfaction' (*i.e.*, abolish the 'set') unimpeded by other reactions that cannot be readily absorbed by (integrated with) them, there is no emotion. Emotion arises only when these other reactions (implicit or overt) are so irrelevant as to resist ready integration with those already in orderly progress towards fruition. Such resistance means actual tension, checking, interference, inhibition, or conflict—conflict between the intercurrent reactions appropriate to the original 'set' and reactions of other origin threatening the abortion or frustration of the relevant series. Such conflict constitutes the emotion".

[79] Kant's terminology—but 'image' is used in the sense of 'effective stimulus' external or internal.

[80] *The Theory of Emotions, pp.* 562 *et seq. Psych. Review*, vol. 1, *no.* 6, 1894, and *pp.* 27 *et seq.* vol. 2, no. 1, 1895.

[81] *op. cit. p.* 29.

highly reflex action to the 'attitude' which is indicative of possible choices of alternative responses by the individual. In emotion the control of behaviour is placed more completely within the organism and is less fully 'forced' by the environment. The general tendency is toward contemplation and away from unreflective overt behaviour. James' hypothesis of emotion as[82] "*weakened repetitions of movements which formerly* (when they were stronger) *were of utility to the subject*," adds another argument to this view. He held that, on the evolutionary hypothesis, one might assume that overt action would undergo diminution and that the process would leave traces. In the evolutionary development of man, for example, the capillary action of the hair, functioning to make the individual appear larger to his enemy, must at one time have been an important mechanism of defence. As such it still functions for less highly developed animals and has indeed certain practical application in primitive warfare.[83] However, the phrase 'weakened repetition' is probably inaccurate. The emotions are, rather, a retention in the evolution of man of "a stage in their development simply because it has never been lost".[84] In this sense emotional attitudes are "survivals" rather than "phyletic contractions"[85].

It is possible at this point to revert to Cannon's hypothesis that the emotions are teleologically 'preparation for emergency action'. Emotional stress, he suggests, is characterized by the co-operation of adrenin 'with sympathetic nerve impulses in calling forth stored carbohydrate from the liver, thus flooding the blood with sugar; it helps in distributing the blood to the heart, lungs, central nervous system and limbs, while taking it

[82] *Principles of Psychology*, vol. 2, *p.* 478.

[83] *Cf. Tacitus, de Germania*, xxxviii, "Among the *Seubi*, however, even till old age they continue to fasten back their unkempt hair, and often they knot it on the very top of their head. This comes from their care for their personal appearance, but it is not mere vanity; for they do not adorn themselves in order to enter the lists of love, but they thus add to their height that they may appear more terrible to the eyes of the enemy when going into battle."

[84] T. H. Morgan, *Critique of Evolution*, *p.* 21. The author is discussing the physiological retention of primitive organs, *etc.*

[85] Morgan *ibid.*; *vide* Landis, *The Expressions of Emotion* in *Handbook of General Experimental Psychology*, *p.* 346.

away from the inhibited organs of the abdomen; it quickly abolishes the effects of muscular fatigue; and it renders the blood more rapidly coagulable'.[86] These are purely and simply mechanical reactions. But, in addition, Cannon admits 'many surface manifestations of excitement':[87]

> The contraction of blood vessels with resulting pallor, the pouring out of 'cold sweat', the stopping of saliva-flow so that the 'tongue cleaves to the roof of the mouth', the dilation of the pupils, the rising of the hairs, the rapid beating of the heart, the hurried respiration, the trembling and twitching of the muscles, especially those about the lips—all these bodily changes are well recognized accompaniments of pain and great emotional disturbance, such as horror, anger and deep disgust.

These peripheral manifestations, as has been seen, Cannon regards as unimportant. They can be unimportant only because a) they are not behaviour, which is obviously incorrect or b) they have no function. The arguments against the latter objection have been summarized. However, on the view just propounded, the capillary action of the hair, dilation of nostrils, *etc.* are complete in themselves, survivals and not contractions. They are contractions only in the sense that they, in the evolutionary process, no longer function directly as means to end, as do the instincts, in relation to environment. And since they are total reactions—working under the 'all or none' hypothesis, which Cannon himself cites with approval—it is obvious that emotions may not be defined as preparation for emergency action. The emotions are emergency action.

The emotions are then closely related to the instincts as a portion of the organism's *appercepzionsmass*. They are differentiated from the instincts in that they involve the capacity of the organism to present itself with alternative 'images' or effective stimuli. They are important epistemologically through their alliance to contemplation. Practically they function to allow increased flexibility into otherwise reflex action and involve an

[86] *Bodily Changes*, *p.* 193.
[87] *Ibid.*, *pp.* 2–3.

emphasis upon the individual's increasing freedom from the control by nature and the substitution of control to nature.

The end process, that portion of the continuum known as 'mood', presents at once the antithesis to the instincts and the development of the factors of 'imagination' apparent in the emotions. The stimulus induces similar physiological and neurological preparation for action and the *appercepzionsmass* functions as it does in the emotions and instincts. But to the organism, the stimulus has become a total situation in which vastly more 'images' or effective stimuli, for future action are apparent. Conversely, the overt action characteristic of the instincts is inhibited and replaced by contemplation of the alternative "images", or effective stimuli. At this point in the continuum, there result association of images and what Darwin called "reacting similarly to analogous-feeling stimuli".

Bryn Mawr College, Bryn Mawr, Pa.

DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

CONCERNING THE STRUCTURE OF WHOLES

Dear Sir:

In his suggestive article on the Structure of Wholes[1] M. Angyal considers it a desideratum to "construct a logic of systems". Providing we are right in assuming that his notion of a system (or "static whole") is very closely related to the concepts of *complex* and of *gestalt* as we have defined them in a recent paper[2], we have good reasons to believe that we have accomplished M. Angyal's desideratum to a fair extent. Thus a brief summary of our results may be of some interest to your readers.

A good example of a *complex* is to be found in a sequence of musical tones, each of them with a definite duration and intensity. This complex can be characterized as a distribution of the *functions of state:* pitch, intensity and quality over the period of time during which the sequence is being produced. This period we call the *domain of positions* of the complex. Then the complex defines the value of each function of state (pitch, intensity, quality) for every point of the domain and is itself entirely defined by this fact.

Furthermore we introduced the notion of *transposition,* defined as a certain kind of one-one relations between complexes; an illustration was furnished by what is usually called "transposition" in musical theory. Let us compare two sequences of tones the second of which being obtained by a transposition (say, of an octave) of the first. The tones of the first can be correlated to those of the second in such a way that a strictly definable relation holds between the pitches of correspond-

[1] Vol. 6, no. 1 of this review.

[2] K. Grelling und P. Oppenheim: *Der Gestaltbegriff im Lichte der neuen Logik,* "Erkenntnis", vol. VII, p. 211–225 and p. 357–359.

ing tones. Our concept of transposition is a generalisation of the musical one.

A property of a complex which remains unchanged in a transposition is called an *invariant* with respect to that transposition. In the case of the sequence of tones *melody* is an invariant with respect to all transpositions (the word taken in its musical meaning).

The melody of a sequence of tones is an example of a *gestalt*, and in a general way we define the gestalt of a complex (with respect to certain transpositions) as its invariant with respect to these transpositions.

Now it seems to us that not only what M. Angyal calls a *system* or a static whole may be identified with our *complex*, but also that his *organization* may be our *gestalt* and his *dimensional medium* our *domain of positions*. Perhaps his *internal qualities* and similar expressions correspond to our *functions of state*. Of course in order to determine the adequateness of these translations M. Angyal's terms ought to be worked out in a much more detailed manner.

In the last paragraphs of his article M. Angyal speaks of *dynamic* wholes as opposite to static ones. We believe that this notion can be identified with what we called a *determinational system*. We expressed the opinion that this notion has nothing to do with gestalt as defined by us although unfortunately the two terms are interchanged often. The notion of determinational system was provisionally defined in our paper mentioned above. A fuller discussion of this topic will be presented by us at the Fifth International Congress for the Unity of Science (Harvard University, Sept. 3–9, 1939).

KURT GRELLING
PAUL OPPENHEIM

21, Avenue Victoria
Brussels, Belgium

Dear Sir:

I have read with great interest the above discussion by Messrs. Grelling and Oppenheim and also the article[3] to which they refer. I feel that the topic they are dealing with, i.e., the logical clarification of the concept of gestalt, is highly significant. At the present time, however, I fail to see a very close relationship between the point of view presented by them and the ideas expressed in my paper. First of all I miss in their paper the fundamental distinction between the two logical

[3] K. Grelling und P. Oppenheim: Der Gestaltbegriff im lichte der neuen Logik "Erkenntniss", Vol. VII, p. 211–225, 1938.

categories of "relationship" and "system" which has been the crucial point in my article.

Messrs. Grelling and Oppenheim have tentatively translated certain terms which I have used in my article in their own terminology. Indeed it seems to me that the facts we are referring to with different terms are more or less the same, but I am still doubtful whether the terms used by them on the one hand, and by myself on the other, have the same theoretical implications. Both papers are somewhat short and condensed and I feel that they do not permit a fruitful discussion at present. I am looking forward with considerable interest to the more detailed discussion of this topic which Messrs. Grelling and Oppenheim have announced.

Andras Angyal

Research Service of the Worcester State Hospital, Worcester, Mass.

REVIEWS AND NOTES

Theory of Valuation. By John Dewey. International Encyclopedia of Unified Science, Vol. II, Number 4. The University of Chicago Press, 67 pages, $1.

The problem of valuation is seen as closely associated with the problem of the structure of the sciences of human activities and human relations. Valuing means both prizing and appraising.

Value-expression as merely ejaculatory is discussed as an extreme unacceptable view. Valuation as liking and disliking is to be considered in terms of observable and identifiable modes of behavior. As behavioral the adjective 'affective-motor' is applicable although care must be taken not to permit the "affective" of quality to be interpreted in terms of private "feelings"—an interpretation that nullifies the active and observable element expressed in 'motor'. It is futile to try to assign signification to words in isolation from objects as designata. Desires arise only within certain existential contexts. There is no desire at large.

As to propositions of appraisal the conclusions reached may be summarized as follows: (1) There are propositions which are not merely about valuations that have actually occurred (about, *i.e.*, prizings, desires, and interests that have taken place in the past) but which describe and define certain things as good, fit, or proper in a definite existential relation: these propositions, moreover, are generalizations, since they form rules for the proper use of materials. (2) The existential relation in question is that of means-ends or means-consequences. (3) These propositions in their generalized form may rest upon scientifically warranted empirical propositions and are themselves capable of being tested by observation of results actually attained as compared with those intended.

The connection between ends and values is illustrated by the fact that propositions in which things (acts and materials) are appraised as means enter necessarily into desires and interests that determine end-values. There is a continuum of ends-means. Science is itself a value and is also the supreme means of the valid determination of all valuation in all aspects of human and social life.

Prof. Dewey's study is very valuable from the viewpoint of presenting the conditions which confront *all* theories of valuation. W. M. M.

Beyond Yonder. By O. J. Lee. Chapman & Grimes, Boston, 169 pages, $2.50.

A very popular exposition of astronomy with stress on knowing and overcoming *distance.* Competent and well illustrated. W. M. M.

La Logique D'Une Idée-Force. L'Idée D'Utilité Sociale Et Révolution Française (1789–1792). Par Jean Belin, Hermann et Cie, Paris, 635 pages 120 fr.

Historical, juridical and philosophical considerations applied to a complex notion, namely the idea of social utility, as illustrated in a period of reform. Also available in 5 separate numbers. Thoroughly annotated and of greatest value to historians. W. M. M.

Communication. By Karl Britton. Harcourt Brace & Co., International Library of Psychology, Philosophy, and Scientific Method. Price: $3.75.

Despite the fact that this treatise does not claim to be metaphysical, our review will criticise the metaphysical implications of Mr. Britton's linguistic theory. Take the very first discussion—what is communicated? "Words are used to communicate thoughts; words mean thoughts or successions of thoughts." But is this strictly so? When one says "The book is on the table" is he communicating a thought? Is he not rather conveying something about objects or processes in a given situation? Does Ohm's Law state anything about the thoughts of men? Stating his solution in this fashion, Mr. Britton then has the further problem of relating thoughts to what they are thoughts about, as well as relating words and thoughts. The simplest way to meet this problem is to discover the actual connections between what we say and what we talk about. This problem leads to that discussed in chapter 8 "Structure of Language and Structure of Fact."

This problem appears to be the core of the book, although we confess to find little here that has not already been proclaimed by other British empiricists. Once more we find the totally uncalled-for "world of facts" with its corollary "correspondence." Mr. Britton makes a familiar but dubious distinction between facts and events; the former being of a logical order and the latter of spatio-temporal order. Events are not differentiated into kinds. Structure is imposed upon data.

A structured fact world is interposed between events or objects and the instrument of language. Our processes of discovery, imposing structure upon events, erect a world of facts which language mirrors. This world of facts is said to have a general structure which is revealed in the formal laws of language. A distinction is made between such discoveries about the general modus operandi of language and reality as such. Reality as such (the realm of physical events) is conceived as excluding the man-made instrument of language.

Empirical generalizations are conceived as non-factual statements which are merely convenient (or not) usages to adopt in talking about events. They are about facts, not events. Here again no examples of actual scientific laws are brought forth as evidence and the discussion is carried on in terms of the blanket form "All S is P" which is said to be general for all empirical laws. The basis of this view seems to be Poincaré's remark that general laws are conventional in the sense of appropriate and not arbitrary in the sense of not completely independent of any empirical basis. No attempt is made to justify this by analysis of the way in which scientific laws do in fact function.

A further metaphysical implication of Mr. Britton's theory of language is that his analysis of propositions is not functional or contextual. For example, the author suggests without referring to the context that the proposition "The King of France is bold" is a false proposition. If the context were known to be that of the present French government, the proposition would be *meaningless* and not merely false. In the context of pre-revolutionary France, on the other hand, the proposition is meaningful and its truth or falsity can be discovered.

Mr. Britton approaches the problems of communication with the traditional empiristic sharp distinctions between emotion and intelligence, the physical and the logical. His aim is to explain different kinds of language:—empirical propositions, necessary principles, moral and ethical judgments, and poetical expressions in terms of two functions—the informative and emotive. The first two are informative respectively of events, existences, and of our linguistic habits. The latter two merely express our emotions; to think otherwise is to commit the pathetic fallacy. When one investigates language, he does not find that these two methods of speaking are really so diverse. Mr. Britton does not distinguish these types of sentences upon the functions they perform, but only upon their subject matter. Metaphors, similes, etc., are not used solely in poetry, for scientific language also is metaphorical and imaginative. Poetry does not express something about the feelings of men; it expresses the same things as science does in a different way.

In fine, this book does not represent any essential advance over "The Meaning of Meaning," since it accepts without criticism the fundamental distinctions of that book and repeats its errors. Mr. Britton reverts to the old tricks of the empiricists in his theory of contingent propositions, in his analysis of propositions, and in the chapter on necessary propositions. Greater metaphysical value would have been derived had the book been written with these suggestions in mind: what factors are necessary in order for any people in any society to communicate about anything? Then list the factors, such as, conventions, relations between language and its subject matter, the rôle thinking plays, the physical modes for communicating, and a differential analysis of the kinds of signs there are. Our philosopher would then tackle the problems in succession, showing the rôle each factor plays in the process of communicating. In this manner one might at least attain continuity and co-ordination, which are sorely lacking in Mr. Britton's book. It would also have the virtue of contributing something new to this muddled field of the philosophy of language.

George J. Bowdery
John R. Bross

Author Index

Ablowitz, R., 1
Angyal, A., 25
Benjamin, C. A., 422
Cook, T. I., 318
Copilowish, I. M., 181
Darwin, C. G., 48
Dexter, L. A., 404
Fries, H. S., 301
Hardie, C. D., 309
Hempel, C. G., 163
Hofstadter, A., and McKinsey, J. C. C., 446
Kluckhohn, C., 328
Lesser, A., 345
Malisoff, W. M., 17, 127, 257, 261, 378, 379, 380, 381, 382, 385
McCrea, W. H., 137
McGill, V. J., 196
McKinsey, J. C. C., and Hofstadter, A., 446
Nagel, E., 212
Nahm, M. C., 458
Peddie, W., 38
Ramsperger, A. G., 390
Ratner, S., 88
Seifriz, W., 266
Sellars, R. W., 412
Vivas, E., 432
Werkmeister, W. H., 356
Winn, R. B., 285

Subject Index

Ambiguity, Border-line cases, Vagueness, and, 181
Anthropological Studies, The Place of Theory in, 328
"As If" in Physical Science, The Philosophy of, 38
Atom, What is an, 261
Causal Imputation and Purposes of Investigation, 404
Correspondence, 98, 254, 367, 487
Critical Realism, A Clarification of, 412
Cultural Indices, Natural Languages as, 356
Culture in History, Patterns of, 88
Culture, Research Procedure and Laws of, 345
Discussion (Correspondence), 98, 254, 367, 487
Emergence, The Theory of, 1
Emergence Without Mystery, 17
Emotion, The Philosophical Implications of Some Theories of, 458
Excluded Middle, Concerning the Laws of Contradiction and, 196
Fact, Value and, 432
Gene, What is a, 385
History, Patterns of Culture in, 88
Imperatives, On the Logic of, 446
Investigation, Causal Imputation and Purposes of, 404
Knowledge, Probability and the Theory of, 212
Knowledge, What is Scientific, 390
Languages as Cultural Indices, Natural, 356
Life, A Materialistic Interpretation of, 266
Logic of Imperatives, On the, 446
Logic, Vagueness and, 163
Materialistic Interpretation of Life, A, 266
Minds, Our Knowledge of Other, 309
Nature Rational, Is, 285
Physical Science, The Philosophy of "As If" in, 38
Physics, A Vicious Abstraction, 301
Physics, Probability, many-valued logics, and, 65
Probability and the Theory of Knowledge, 212
Probability in Physics, Logic and, 48
Probability, Many-Valued Logics, and Physics, 65
Realism, A Clarification of Critical, 412
Research Procedure and Laws of Culture, 345
Reviews, 115, 257, 378, 490
Science: Natural and Social, 318
Scientific Knowledge, What is, 390
Space-Time and Mechanics, The Evolution of Theories of, 137
Vagueness, and ambiguity, Border-line cases, 181
Vagueness and Logic, 163
Vagueness, Science and, 422
Value and Fact, 432
Virtue and the Scientist, 127
Wholes, The Structure of, 25

DATE DUE

UIC MAR 26 '87
RET'D PER MAR 2 3 1987

PERIODICALS MUST BE RETURNED
TO PERIODICALS DESK ONLY

DEMCO 38-297

THE UNIVERSITY OF ILLINOIS AT CHICAGO
3 8198 318 729 595